MEDICAL REPOSITORY.

VOL. III.—No. II.

ARTICLE I.

Two Caſes of the human Conſtitution being affected by the Contagion of Small-pox and Meaſles, at the ſame Time. In a Letter from Dr. PHILEMON TRACY *to Dr.* MITCHILL, *dated Norwich (Connecticut), July* 19, 1799.

IN the ſpring of the year 1797, being then engaged in inoculating for the ſmall-pox, two caſes fell under my care, which exhibited unequivocal evidence of the poſſibility of two diſtinct diſeaſes ariſing at the ſame period in the human frame, and each purſuing its ordinary courſe as when ſeparately exiſting, attended with all their uſual characteriſtic marks; and, though I am ſenſible that the weight of medical opinion may militate againſt my experience on this queſtion, ſtill the concluſive evidence that the facts afford, has removed every doubt that previouſly exiſted in my mind on contemplating the ſubject, and leads me to cheerfully ſubmit the caſes to the candour of the faculty.

Caſe 1. W. T. a young man, applied for admiſſion into my hoſpital for inoculation, and mentioned, at the ſame time, his having been expoſed, a day or two previouſly, to take the meaſles, which excited ſome anxiety in his mind reſpecting the ſafety of receiving the ſmall-pox, under the liability of being affected with the meaſles at the ſame period. Thinking this a good opportuntity to determine, whether two ſpecific contagions could operate at the ſame time on the human frame, and concluding no great danger would attend the experiment, I received him into the hoſpital, and inſerted the variolous matter, from a well ſuppurated puſtule, in the uſual manner. The local inflammation, at the part inoculated, came for-

ward at the common period, with as much activity as occurred on the other patients, at the ſame time inoculated; which was followed by the precurſory eruptive fever, on the eighth day from inoculation. The ſymptoms were mild, and continued to the tenth day, at which period a number of diſtinct puſtules were viſible around the place inoculated, and many were diſcoverable on the face and neck, juſt emerging from the ſkin; the eruptive ſymptoms continued ſtill in a moderate degree. The mild form which the variolous diſeaſe at this time aſſumed, greatly relieved my patient from his anxiety, ariſing from the fearful apprehenſion of being jointly attacked with two ſo formidable diſeaſes, as the ſmall-pox and meaſles, at the ſame period. I left him in this ſtate of tranquillity; but in about four hours after I was called to viſit him, and found him labouring under ſevere pain in the head and loins, attended with rigors, pyrexia, &c. As he had not expoſed himſelf to take cold, or been guilty of any marked imprudence, I immediately ſuſpected him affected with the premonitory ſymptoms of meaſles; and, in conformity with this belief, adopted phlebotomy, with the antiphlogiſtic method generally. On the next day the meaſley efflorescence made their appearance on the ſurface, attended with cough, coryza, and all the other uſual marks of this diſeaſe, which progreſſed in the common manner to a favourable iſſue. During this period, from the firſt acceſſion of the meaſley ſymptoms, the local inflammation at the part inoculated continued bright, and the previous puſtules not only remained viſible, and progreſſing towards maturation, but a number of additional puſtules actually appeared on the lower extremities, eaſily diſtinguiſhable by their hardneſs and prominency, and which maturated in the uſual manner, as when ſeparately exiſting, except that the ſuppuratory proceſs ſeemed leſs rapid than in many other caſes: thus, hand in hand, theſe two diſorders proceeded to a favourable termination, which freed my patient from his great ſolicitude, and impreſſed me with the belief, that different ſenſitive principles of the animal frame may be morbidly excited, from different cauſes, at the ſame period, each equally productive of its peculiar form of diſeaſe.

Caſe 2. J. S. after being expoſed to the ſmall-pox, in the natural way, was ſeized, at the uſual period, with the ſymptoms of the diſorder, which were followed by a puſtular eruption on the ſurface; and, on the next day from their firſt appearance, was attacked with ſymptoms of a ſimilar aſpect

to thoſe which ſupervened in the former caſe, though in a more aggravated degree, which I treated as in the former inſtance, and which were followed by a univerſal meaſley eruption on the third day, with the uſual concomitants. During this period, the previous puſtular eruption, which was copious, remained bright and prominent, and new puſtules continued to appear on the lower extremities, all of which proceeded to maturation in the uſual manner; while the meaſles purſued their ordinary courſe, neither diſeaſe ſeeming to retard the other in its progreſs, but, like two friendly ſojourners in ſeparate apartments of one tenement, ſeemed mutually diſpoſed to purſue their different careers, without officious interference or moleſtation to each other. This caſe, like the other, terminated happily. No room was left to doubt the identity of the meaſles in either caſe, as neither of the ſubjects had previouſly been affected with that diſorder: both had been expoſed to take it; and a conſiderable number of my patients at the hoſpital, without having been ſenſibly expoſed to the diſeaſe in any other way but from theſe patients, were affected at the uſual time with this diſorder.

Feeling myſelf greatly incompetent to explain the variant ſenſitive principles in the human machine, which are ſubject to be conjunctly excited from different cauſes, each productive at the ſame time of the phenomena, peculiarly marking diſtinct diſorders, I ſhall avoid the attempt; but this belief is ſtrongly impreſſed on my mind, that two or more ſpecifically different diſorders may ariſe at the ſame time in the human frame, and purſue their natural courſes, notwithſtanding the zeal with which the Brunonian advocates attempt to ſupport the untenable theory of the unity and indiviſibility of their principle of excitability pervading the ſyſtem at large, and being ſuſceptible of only an individual morbid excitement at a time.

ARTICLE II.

An Account of Diſeaſes prevalent at Londonderry in the Years 1797 *and* 1798; *communicated in a Letter from Dr.* WILLIAM PATTERSON *to Dr.* MILLER.

[Continued from p. 46, and concluded.]

INTESTINAL FEVER.

A DISEASE appeared in November, 1797, and diſappeared in February, 1798, which might be called a dyſenteric or inteſtinal fever, as it commenced, and generally proceeded, with ſymptoms of pyrexia, attended, in every caſe, either conſtantly or at intervals, with affections of the bowels, ſuch as pain, diarrhœa, mucous ſtools, and teneſmus, not often with blood. The ſubjects of it, in its ſevereſt degree, were principally children, from two to ten years of age, and thoſe almoſt entirely females, few or no males having been affected with it, at leaſt in any obſervable meaſure.

The manner in which adults were affected was, firſt, by a ſlight feveriſhneſs and ſharp gripings, which, in moſt inſtances, were ſoon followed by a copious diarrhœa, that generally terminated the complaint in three or four days; and the more early and abundant this diſcharge, the more expeditious and perfect was the recovery. Very few adults were long affected; and when they were, it was only in thoſe caſes where the diſtemper partook moſt of the dyſenteric nature: but in the worſt of theſe caſes, though emaciation was conſiderable, the bodily ſtrength was not proportionably reduced.

In the young patients, although the teneſmus was often urgent, yet little blood appeared in the ſtools; but ſometimes two or three table-ſpoonsful of this fluid, pure and unmixed, were evacuated from the anus, and with evident relief of the inteſtinal diſturbances. The pulſe, in theſe patients alſo, was from 90 to 120 in a minute; quickeſt when the febrile ſtate prevailed moſt; but ſeldom was it found to have any ſort of fulneſs. Near the commencement of the diſorder, the thirſt was, in general, greateſt: it often remitted in the courſe of the malady: ſometimes it ceaſed; but it was governed by the temper of the fever. The tongue was uſually mellow, and not more changed than by being ſometimes covered with a thin pellicle of whitiſh fur.

The head ached at times; but in the main it was affected

with wandering pains; and ſimilar complaints were now and then referred to the trunk and extremities, eſpecially towards the departure of the diſeaſe, and when it had manifeſted moſt of the febrile propenſity, without free evacuation from the bowels.

The ſkin was almoſt univerſally dry, ſometimes hot, ſometimes cold, and, at other times, in its natural temperature; the greateſt degree of heat, as might be expected, taking place in the ſevereſt paroxyſms of fever. The appetite was early impaired, and continued very defective throughout the whole of the diſorder, until the dawn of amendment appeared, when the food became more grateful, and afterward the natural deſire for it gradually increaſed; but in the pureſt dyſenteric caſes it returned the moſt ſlowly, and was the moſt inconſtant, keeping pace with other movements towards recovery.

Until the diſeaſe began to diminiſh, and eſpecially in the exacerbations of fever, the ſleep at night was diſturbed and unſatisfactory; during which a good deal of incoherent muttering was expreſſed; but at no time did any permanent delirium, or unconſciouſneſs of mind, ſtupify the patient. In the febrile predominancy, the ſtrength was moſt reduced, but ſooneſt reſtored; in the dyſenteric, it was variable, but ſeldom depreſſed, except at the onſet and recurrence of pyrexia.

In moſt caſes, after a few days in the beginning, there were clear remiſſions; and ſometimes there were evident intermiſſions, for two, three, or more days, promiſing to end ſoon in recovery, but in that reſpect proving deceitful. The duration of the diſeaſe was from two to five or ſix weeks; the moſt purely febrile modification being of the ſhorteſt, and the moſt unmixed dyſenteric form being of the longeſt continuance; whilſt the caſes in which diarrhœa occurred were the moſt abbreviated and eaſy of all.

In one caſe the tongue, membrane of the cheeks, and of the gums, ſwelled, inflamed, and degenerated into ſmall ulcerations, which were ſoon healed by the uſual methods, and which ſeemed to act, in ſome meaſure, as a critical ſolution of the diſeaſe. In no other inſtance did I obſerve any eventful exertion, eſpecially tending to curtail the diſorder, if we except the diſcharge of blood above-mentioned, though this ſeemed rather to lead towards an amendment, than to operate as a tranſlated action, or criſis of the diſtemper.

The moſt ſucceſsful *method of treatment*, in the febrile

preponderancy, was calomel purges, neutral mixtures, cool air, cold drinks, and every branch of what is termed the antiphlogiſtic regimen: in the dyſenteric aſcendancy, caſtor oil (found the moſt ſalutary laxative), enemas, milk, eſpecially butter-milk, ſucculent fruit, vegetable aliment, well-ventilated apartments, and great cleanlineſs in every particular. Antimonial emetics were uſed at different ſtages of both modifications of the malady, but not with any obvious advantage. When the dyſenteric character prevailed, pulv. ipecac. comp. was adminiſtered, yet it was not followed by any viſible good effect. The cinchona, which, at ſundry times, ſeemed to be particularly indicated, inſtead of anſwering the intention, manifeſtly diſagreed with the patient, or appeared to be perfectly neutral: nor had aqueous preparations of colomba and gentian roots more curative operations, but were not as unfavourable, or indifferent medicines, as the cinchona. Where ſtomach debility ſeemed to retard recovery, and where the circulation and the bowels had become placid, the ſulphuric acid was called into aid, but was not found a contributive article. In ſhort, a well-regulated diet, with due attention to the other non-naturals, as they are termed, was the beſt aſſiſtance in the convaleſcent ſtate, and in expediting the return of uſual health.

Under this management, which I ſoon adopted, I did not loſe a patient; nor did I hear of any fatality cauſed by this diſorder in the town. In the country, however, although it was there generally more mild, ſome fatal inſtances occurred; but theſe, I underſtood, were occaſioned by prepoſterous treatment of the feveriſh ſymptoms. On the ingreſs of this epidemic, I was at ſome loſs to decide upon its real nature, as it aſſumed a diverſity of feature and form; but by early and attentive obſervation, I ſoon diſcovered its intrinſic character, and acquired a criterion to judge of the moſt rational way of treating it. This ſort of dilemma, I believe, is the inevitable fortune of a practitioner at the entry of moſt epidemic diſtempers: it was the caſe of the great Sydenham; and ſhall we hope, or preſume, to be more promptly ſagacious and comprehenſive than the Britiſh Hippocrates?

As the febrile caſes, in a few patients, were the moſt urgent and intereſting, I ſhall ſubjoin one of the more important and inſtructive, from its putting on ſome of the appearances of that treacherous diſeaſe, *hydrocephalus internus*, or rather, *febricula hydrocephalica*, in its firſt and moſt wily ſtage.

Case.

On Wedneſday evening, the 17th of January, 1798, I firſt viſited Miſs ——, aged five years, and found her ſitting, with her head leaning on a perſon's breaſt. Her pulſe was quick, 106 in a minute; ſkin dry; breathing moderate; countenance heavy, yet fluſhed. Her eyes were not dull, nor morbidly affected in other reſpects; but were perfectly obedient to different degrees of candle-light, although ſhe had particularly complained of pain in them. Tongue but little furred. She had not had any material vomiting, but was affected by a degree of retrograde motion about the pharynx, whenever ſhe took any ſort of liquid.

The account which I received of the beginning and progreſs of the ailment was, that the child firſt complained of pain in her bowels, and, eight days before I ſaw her, was attacked with diarrhœa, for which ſhe took no medicinal article, except ſome rhubarb; and that the diarrhœa continued troubleſome only four or five days; but when it abated, that ſhe appeared to grow worſe inſtead of better.

The family apothecary had ſeen her the day before I ſaw her, and had given her one or two grains of calomel, and ſome ol. ricini, without any viſible effect. I ordered a cantharides plaſter to be laid over the ſcapulæ, and when this was applied, to adminiſter an emolient injection. Whilſt the plaſter was applying the child grew faintiſh, which greatly alarmed her parents, but from which ſhe ſoon recovered.

18th. Morning. Slept uneaſily laſt night, though her reſt was better than in ſeveral preceding nights. Pulſe 120, ſharp, with a crepitous feel; ſkin dry; no evacuation by the enema. In the afternoon the pulſe was not altered; nor was it changed at the beginning of the night, except being leſſened in number two ſtrokes or beats, and it was not ſo crepitous; mouth was tinged with a waſh of red wine, which I prohibited, and, inſtead of it, ordered one compoſed of water, lemon-juice, and ſugar. Ordered ſix grains of calomel, and ſix of jalap, to be well triturated together; then to be divided into three doſes, and one to be given every ſecond hour, till they operated. Soon after the ſecond doſe another ſick fit came on, but it was of ſhort duration. Two or three looſe motions, mixed with frothy mucus, followed the uſe of the ſecond doſe of the laxative powder. The bliſtering plaſter operated thoroughly. —Before I attended, the child drank reluctantly during the

day, but with more freedom at night; and, in general, ſince I ſaw her, ſhe took drink with much leſs repugnance.—Squalls often to-day, yet the ſound is not in a ſhrill ſcreeching tone—toſſes a good deal, but can lie equally eaſy on either back or ſides. Her eyes continue to look healthy—made more water laſt night than before at any period—ſleeps compoſedly and breathes eaſy. She had been much lightened of complaints this morning, but ſhe did not long continue ſo. Complained of pains in different parts; ſometimes in her head, ſometimes even in her feet, but moſtly in the abdomen, which is much contracted.

19th. Morning. Though ſhe reſted but indifferently laſt night, her pulſe does not exceed 104, is ſofter and more equable, and free of the crepitous ſenſations—aſked for a little food—drinks better than yeſterday. Her limbs had been ſeldom cold, nor did ſhe complain of chilline ſs at any period of the diſeaſe, although ſhe was at firſt obſerved to creep towards the fire. At night her pulſe was nearly the ſame as in the morning—had two eaſy motions, without any material pain, and of a more healthy colour and conſiſtence. When ſhe ſqualls today it ſeems to proceed from a petted and peeviſh ſtate of temper, more than from an incitement of bodily pain—ordered to continue the diluting cool drink, viz. butter-milk, thin gruel, and plain water; alſo a doſe of kali citrat. dilutum, every ſecond hour, and to have the enema repeated at night. She ſat out of bed better and longer to-day than yeſterday, being at the ſame time more attentive to things around her—eyes look well.

20th. Morning. Reſted better laſt night—had a moiſture over the ſurface—during the night drank above a quart of butter-milk, which is her favourite drink—appetite for ſolids ſtill bad—has had four motions, charged with frothy ſlime—pulſe 98, and communicates that kind of ſubſiding feel to the finger, ſo ſenſibly to be diſtinguiſhed in the decline of ſebrile diſeaſes, eſpecially in that of typhus.

21ſt. Morning. Night's reſt ſtill better, though ſhe had twelve ſtools ſince yeſterday morning—pulſe 104—ſome more tokens of returning appetite. Notwithſtanding the pulſe is quicker than yeſterday, yet ſigns of recovery, in other reſpects, are increaſed.

22d. Morning. Reſted very well laſt night—appetite much better—thirſt nearly vaniſhed—pulſe 96, and its *ſubſidence* more ſenſible—little of the citrated kali was taken. All medi-

cines are omitted, and reſtoration left to the cautious management of diet.

25th. Afternoon. Pulſe 106—was more uneaſy, and ſeemed more feveriſh in the opinion of the family the two preceding days than this day—has had ſome ſlimy motions—thirſt little, ſometimes complains of pains in the hypochondriac and hypogaſtric regions—reſts pretty well at night. Ordered a doſe of oleum ricini.

28th. Afternoon. Pulſe 116—tongue clean—appetite tolerable—no thirſt—reſts well—bowels leſs irregular. Ordered a doſe of oleum ricini in caſe it be requiſite. This evening came on a ſevere trembling, likened to a febrile rigor, and ſuppoſed to be due, partly to an increaſe of fever, and partly to ſeeing a favourite parent for the firſt time ſince her illneſs.

31ſt. Afternoon. Pulſe 104—ſleeps remarkably well at night—bowels more natural—has ſtill ſome evening increaſe of fever—complains of tranſient pains in the head and ſide. Oleum ricini, laſt preſcribed, was not given, as the bowels continued in a better ſtate.

Henceforth the child amended progreſſively, but with a degree of evening exacerbation for three or four days; after which, in eight or ten days more, ſhe was perfectly reſtored to the fond wiſhes of her anxious and affectionate parents.

Jaundice.

Upon the retreat of the inteſtinal fever, the jaundice ſucceeded; and, like the former, was chiefly confined to the younger ages; but its ſubjects were not ſo young as thoſe of its predeceſſor, being chiefly patients at the ſtate of adoleſcence, or arrived at puberty; differing likewiſe in another circumſtance, namely, that of not being principally confined to the female ſex. A few perſons farther advanced in life, had a malady, putting on an icterical appearance, whilſt it ſeemed to proceed more from an injury in the parenchymatous ſubſtance of the liver, than from any incidental derangement of the bile or its paſſages.

The prevailing icterical affection commenced with diminution of appetite, ſtomach ſickneſs, and laſſitude; which progreſſively increaſed to a certain degree, and then gradually abated. The pulſe was ſometimes quick, ſometimes ſlow; the ſkin and the tunica conjunctiva of the eyes were tinged

with a yellow hue, but ſeldom deeply; and a preſſing thirſt now and then teaſed the patient. In one of the caſes, that of a lady, where the liver ſeemed to be ſubſtantially engaged, the yellowneſs was generally much increaſed by exerciſe; an occurrence that appeared to me remarkable, as I had not before obſerved it in ſimilar diſeaſes—and this patient, though beſet with ſeveral unfavourable ſymptoms, was fortunately cured.

The diſeaſe, in its leading ſhape, was treated with antimonial vomits chiefly; ſome laxatives were occaſionally adminiſtered, and afterwards bitter infuſions were preſcribed, by way of finiſhing the cure. But I have a ſtrong ſuſpicion, that the greater part of the remedies ordered for theſe purpoſes was unneceſſary; as I am inclined to believe, that the diſeaſe was of the nature mentioned by Darwin, Claſs 1, 2, 4, 19; and, if ſo, it would, as he remarks, probably have ceaſed of itſelf in about a fortnight, like a common catarrh, without the aid of medicine. This ſpecies of jaundice he aſcribes to a thickening of the coats of the common bile-duct, by inflammation, or increaſed action of their veſſels, ſo as to prevent the paſſage of the bile into the inteſtines, in the ſame manner as the membrane which lines the noſtrils becomes thickened in catarrh, ſo as to prevent the paſſage of air through them; and this he conſiders a frequent cauſe of the diſorder, eſpecially in children. What more probable time for the occurrence of ſuch a cauſe than in an epidemic ſeaſon, wherein the bowels were eſſentially concerned, accompanied with feveriſh affections?

Catarrh.

In the ſpring of 1797, a catarrhal diſorder was very prevalent, but diſappeared on the approach of warm ſummer weather. It conſiſted in a ſenſitive irritative fever, compoſed of ſlight chilline ſs, head-ache, impaired appetite, ſome thirſt, pain in the eyes, ſtuffing of the noſtrils, heat of the ſkin, and increaſed pulſe. A cough attended; it was ſlight in the beginning of the diſeaſe; and, what ſeems worthy of remark, as the cough increaſed, the other ſymptoms abated, and recovery ſoon enſued. Perſons liable to complaints of the ſtomach, or of the head, found thoſe parts, on this occaſion, more ſeverely affected than other parts of the frame. A child, about ſeven years old, ſubject to irritative fever, accompanied with hot flu ſhings, head-ache, and affections in the eyes, even to ſtrabiſmus, had a notable degree of theſe appearances, whilſt under the influence of the prevalent diſorder.

The treatment was very ſimple; and, in fact, the diſeaſe did not require any other; nothing more being indicated than what is found moſt ſalutary in that ſlight irritative fever, or warm catarrh, called a common cold; namely, diluents, cool apartments, refrigerant remedies, and vegetable aliment. Before concluding, it may be neceſſary to remark, that neither in this diſtemper, nor in the prevalent jaundice, nor in the dyſenteric fever, was the lancet employed: I did not meet a caſe in which its uſe was clearly pointed out: and as the febrile ſymptoms betokened generally more of an inirritative than of an irritative nature, there ſeemed to be good reaſon to prohibit, rather than to command veneſection. Nay, further, as the aſcendant nature of fever, in this country, is typhoid, frequently in its worſt character, that of typhus gravior, or of the ſenſitive inirritative type, we are more ſparing in the employment of general blood-letting than practitioners are in the weſtern world.

Quadruped Distempers.

When the ſubject of epidemics affecting the human ſpecies is under conſideration, it is natural to beſtow ſome thoughts on thoſe maladies which prevail, in the ſame ſeaſon or year, amongſt the inferior ſpecies of animals, inhabitants of the ſame terreſtrial abode; and in this we are ſanctioned by the example of eminent medical obſervers, both ancient and modern, who were led to believe that the cauſes, on theſe occaſions, were, in ſome ſort, congenerous, and that the effects were illuſtrative of one another. In this view, the *cat-diſtemper* of 1797, from its extraordinary expanſion, becomes a ſtriking object of attention. It was univerſal in this kingdom; but I regret that I cannot, from my own obſervation, nor, at preſent, from that of any other perſon, give you aught ſatisfactory concerning it. From what I can learn, it began in the ſouth of Ireland, ſome time in the ſpring of that year, nearly about the time it was obſerved to commence in London; and it gradually extended to other parts of the country, until it traverſed the whole iſland. With us, in the N. W. of Ulſter, its egreſs ſeemed ſynchronous with the ingreſs of our inteſtinal fever, which, conſidering the circumſtances noted in the ſecond number of your Repoſitory, deſerves to be held in recollection. Darwin, Claſs II. Order i. Genus 3. is likewiſe well worthy peruſal, with a view to elucidation of this ſubject.

The ſymptoms of this malady, amongſt thoſe domeſtic quadrupeds, according to what I heard in common converſation, and according to my own ſcanty remarks, did not materially differ from the ſymptoms that appeared both in England and America. As to remedies with us, little or none were employed: the animals were generally left to their fate. In ſome inſtances, I was told, that a countryman in this neighbourhood was ſucceſsful, or ſuppoſed to be inſtrumental in ſaving the creatures' lives by the adminiſtration of emetics. The fatality was extraordinary: in ſome houſes, a number of their cats, kept up by ſucceſſive ſupplies, has been cut off to the amount of ſix or eight in a houſe. What then muſt have been the mortality over the whole kingdom?—It ſeems a fact worth remarking, that of ſix or ſeven cats, great favourites, and highly fed, in a gentleman's houſe in the country, not one was obſerved to be in the leaſt indiſpoſed, whilſt thoſe in the ſurrounding farm-houſes and cottages were ſick or dying.

ARTICLE III.

Dr. Priestley's Reply to his Antiphlogistian Opponents.

No. I.

To the Editors of the Medical Repoſitory.

Gentlemen,

HAVING advanced all that has occurred to me of much moment in defence of the doctrine of *phlogiſton*, I ſhall now, according to my promiſe, diſcuſs the replies of my opponents; and I am happy to find in Dr. Woodhouſe one who is equally ingenious and candid; ſo that I do not think the cauſe he has undertaken will ſoon find a more able champion, and I do not regret the abſence of M. Berthollet in Egypt. Dr. Woodhouſe's objections to what I have publiſhed are contained in the fourth volume of the *Philoſophical Tranſactions of Philadelphia*, p. 452, &c. and your *Repoſitory*, vol. ii. p. 398, &c. Not to tire your readers, I ſhall reply to the ſeparate articles in ſeparate letters, and recite ſeveral new experiments.

The antiphlogiſtians maintaining, as a principal ſupport of their hypotheſis, that the calx of mercury is made by the metal ſimply imbibing pure air, and that it becomes running mercury again by nothing more than the expulſion of that air, I obſerved, that when the calx is revived in inflammable air, a great quantity of that air diſappears, and enters into it. And whereas to this it was replied, that this air does not enter the calx, but, uniting with the pure air expelled from it, forms *water*, I ſhewed that (beſides that no ſufficient quantity of water is produced when it is revived over mercury, like what is found when finery cinder is revived in the ſame circumſtances) in ſeveral of my experiments, in which much inflammable air diſappeared, the pure air expelled from it was found mixed with the remainder, as appeared both by the teſt of nitrous air, and by ſome diſagreeable exploſions which happened in the proceſs.

To this Dr. Woodhouſe replies (Philadelphia Tranſactions, p. 463), that the inflammable air that I made uſe of muſt have been mixed with atmoſpherical air, or an exploſion would not have happened. Now, he ſhould not have charged an old experimenter with ſo very groſs an overſight; and I can aſſure him that all the inflammable air I made uſe of was examined with the greateſt care immediately before the experiments, and was found, by the teſt of nitrous air, to contain no ſenſible proportion of atmoſpherical air. He alſo ſays, page 464, that "when the proportion of the precipi-"tate is large, and the inflammable air ſmall, after the in-"flammable air diſappears, the precipitate will give out its "oxygen, and the air which remains will be diminiſhed by "the teſt of nitrous air." In this caſe, however, there could not be any exploſion.

Since I have received his anſwer, I have repeated my experiments with all the attention that I could give to them, and I ſhall ſtate to you what I obſerved. I heated a quantity of red precipitate in $28\frac{1}{2}$ ounce meaſures of pure inflammable air till it was reduced to $24\frac{1}{2}$ ounce meaſures, and found that, whereas before the proceſs it was not in the leaſt affected by nitrous air, the ſtandard afterwards was 1. 8. ſo that it contained a conſiderable mixture of pure air. I repeated the experiment ſeveral times, and always found pure air mixed with the inflammable, when I had revived any part of the mercury.

Continuing one of the proceſſes till, after the diminution, the quantity of air began to increaſe, there was an exploſion;

but it only raiſed the receiver in which the air was confined about an inch, and recovering its poſition, it broke the earthen diſh in which it was placed. After this I made uſe of a tin diſh, and repeating the experiment, there was an exploſion ſo loud, that a perſon at a conſiderable diſtance was alarmed, and came running to ſee what had happened. The receiver, which was a very heavy one, was blown much higher than my head; but, falling on the graſs, was not broken. After this I thought it unneceſſary to make any more experiments of the kind.

But ſince Dr. Woodhouſe ſays, page 462, that when he revived red precipitate in this manner, the remaining air was not diminiſhed by the teſt of nitrous air, I adviſe him to repeat the experiment over mercury, which I have frequently done, and without having any exploſion. Nor has this always happened when I have uſed water. The reſult of many of my experiments over mercury will be found in the ſixth volume of my *Obſervations on Air*, p. 128, which I ſhall take the liberty to recite; and they were made when I was a convert to the doctrine of the compoſition of water.

"The greateſt difficulty that occurred with reſpect to the "preceding theory of the conſtitution of water aroſe from my "never having been able to procure any water, when I re-"vived mercury from red precipitate in inflammable air, or at "leaſt not more than may be ſuppoſed to have been contained "in the inflammable air. In order to make the experiments "with the ſcales of iron, and that with the red precipitate, as "much alike as poſſible, and that I might compare them with "the greateſt advantage, I made them immediately one after "the other, and with every circumſtance, as nearly as I could, "the ſame. The inflammable air was the ſame in both the "experiments, and both the ſcales of iron and the red preci-"pitate were made as dry as poſſible. They were heated in "veſſels of the ſame ſize and form, and equally confined by "dry mercury. And yet, when I heated the former, water "was formed as copiouſly as I have deſcribed it before, viz. "actually running down the inſide of the veſſel in drops, "though only four ounce meaſures of inflammable air were "abſorbed. But though I heated the red precipitate till eight "ounce meaſures of the inflammable air were abſorbed, and "only three-fourths of an ounce meaſure remained, there was "hardly any ſenſible quantity of water produced, certainly "not one-tenth of what appeared in the experiment with the

" ſcales of iron. There was this difference, however, in the " two reſults: In what remained from the experiment with the " red precipitate, I at this time procured a ſlight appearance of " fixed air, whereas there was none in what remained from " the ſcales of iron. The reſiduum alſo from the red preci- " pitate had in it a ſmall portion of dephlogiſticated air: for, " being mixed with an equal meaſure of nitrous air, the ſtand- " ard of it was 1. 8. In this experiment there can be no " doubt but that the dephlogiſticated air, diſlodged from the " red precipitate, united with the inflammable air in the veſſel; " and, as no water was produced, they muſt have formed ſome " more ſolid ſubſtance, which, in the ſmall quantities I was " obliged to uſe, could not be found."

In making theſe experiments over mercury, we neceſſarily uſe but ſmall quantities of air, and therefore the reſults may not, in ſome reſpects, be ſo much depended upon. But I think it ſufficiently appears from them, that no water was formed in the proceſs, and this the new theory abſolutely requires. Much, however, remains to be done on the ſubject; and as I am now old and cautious, and Dr. Woodhouſe a young man, and more ardent in the purſuit, I recommend the proſecution of it to him.

I think it can hardly be denied that, conſidering the great quantity of inflammable air that diſappears in theſe experiments, the greateſt part of it, at leaſt, muſt enter into the calx. And ſince all running mercury muſt conſiſt in the ſame elements, the ſame principle that (with the addition of water) forms inflammable air, and which we call phlogiſton, muſt paſs through red hot glaſs, when the calx of mercury is revived without addition, by means of heat only.

Some experiments that I have lately made on *ſilver*, *gold*, and *platina*, favour this hypotheſis. All theſe metals give a conſiderable quantity of nitrous air when diſſolved; the firſt in nitrous acid, and the two laſt in aqua regia. And when the ſolutions were evaporated, and the dry reſiduums heated in inflammable air, a great quantity of it diſappeared, and the metals were revived; and yet, by means of the ſame acids, theſe dry reſiduums will yield a great quantity of nitrous air. They muſt, therefore, have acquired, by means of heat only, and this tranſmitted through a veſſel not red hot, the ſame principle that was communicated to them by imbibing inflammable air.

Dr. Woodhouſe ſays, p. 462, " If an ounce of mercury

" abſorbs 362 ounce meaſures of inflammable air, it ought to " give out this air when diſſolved in an acid, or ſome ſub- " ſtance into which it enters as a conſtituent part; but mer- " cury, revived from red precipitate by inflammable air, boiled " in ſulphuric acid, gives ſulphureous gas, and when added to " nitric acid, nitrous air; neither of which contains inflamma- " ble air."

But vitriolic acid air (for I muſt take the liberty to uſe my own terms), certainly contains the ſame principle with inflammable air; for it has the ſame effect on common air, and nitrous may actually be formed from inflammable air; for it is produced by heating the nitrated calx of any metal in inflammable air. If copper be diſſolved in nitrous acid, and the water be expelled to a certain point, there remains a *green ſubſtance* which is not at all deliqueſcent; but, when expoſed to heat, gives out a red vapour. Some of this ſubſtance I heated in 21 ounce meaſures of inflammable air, till the veſſel was filled with red vapour, when it was reduced to ſix ounce meaſures; and, I found that when it was mixed with common air, the ſtandard was 1. 55; ſo that it was almoſt wholly nitrous air. There was in it a ſmall quantity of fixed air, but nothing inflammable in it. It extinguiſhed a candle.

I have formerly endeavoured to aſcertain the proportion of phlogiſton in nitrous and inflammable air, and found it nearly the ſame in both. That this is not far from the truth may, I think, appear from comparing the reſult of two of my former experiments, which I never before thought of doing with this view. When I firſt diſcovered nitrous air, I endeavoured to find what quantity of it would be yielded by the different metals, and found that 20 grains of iron yielded 16 ounce meaſures. When, with other views, I endeavoured to aſcertain the quantity of inflammable air, yielded by malleable iron, I found that 120 grains of it yielded 96 ounce meaſures; and this is exactly the quantity of nitrous air that the ſame weight of iron will give; for 120 is to 96 as 20 is to 16.

Twenty grains of platina gave nine ounce meaſures of pure nitrous air, and 22 grains of gold gave eight ounce meaſures. They, therefore, contain nearly the ſame proportion of phlogiſton; for 20 is to 9 as 22 is to 9. 9. They, therefore, contain little more than half as much phlogiſton as iron; for it will be in the proportion of 170 ounce meaſures to the ounce. It is, however, more than is contained in lead, which

I found to be 108, but leſs than biſmuth, which is 185; and much leſs than mercury, which I made to be 362 ounce meaſures to the ounce.

With thanks for the indulgence with which you have favoured me, I am, Gentlemen,

Your very humble ſervant,

J. PRIESTLEY.

Northumberland, July 18, 1799.

ARTICLE IV.

DR. PRIESTLEY'S REPLY TO HIS ANTIPHLOGISTIAN OPPONENTS.

No. II.

To the Editors of the Medical Repoſitory.

Gentlemen,

PRESUMING that you have received the firſt number of my reply to my opponents, I take the liberty to ſend a ſecond, not doubting your impartiality with reſpect to the diſcuſſion that I have propoſed.

1. Of ſome experiments relating to the conſtitution of metals, and of fixed air.

I have endeavoured to ſhew, by a variety of experiments, that when metals are calcined in atmoſpherical or pure air, ſomething which has obtained the appellation of phlogiſton is emitted from the metal, as well as that ſomething is imbibed by it; that this phlogiſton, uniting with part of the pure air, forms part of the phlogiſticated air that is found after the proceſs, and that, in ſome caſes, it alſo forms fixed air. This muſt neceſſarily be the caſe in ſome experiments with metals, which are not pretended to contain carbone, or plumbago, which are ſaid to be the only ſources of fixed air.

In order to refute my argument, Dr. Woodhouſe makes a compariſon between the calcination of *iron* and of *copper* in dephlogiſticated air. When he calcined 90 grains of bar iron he found an ounce meaſure of fixed air; but he ſays, p. 456, that after the experiment with *copper*, in which 16 ounce meaſures of pure air were reduced to 4, neither fixed nor phlo-

gisticated air was found, and that the remaining air was *perfectly pure.*

Now, as I had always found, that when I calcined any other metal over lime water, a thick scum never failed to be formed upon it, which shews the actual generation of fixed air, and for this purpose had tried *iron*, *zinc*, *lead*, *tin*, *bismuth*, and *regulus of antimony*, of which an account may be seen in your *Repository*, vol. ii. p. 266, I thought it unnecessary to repeat the experiment with any other metals. However, on receiving Dr. Woodhouse's answer, I tried some clean filings of *copper*, and had the same result. A thick scum was formed on the surface of the lime water, which proved the generation of fixed air in the process; and as 33 ounce measures of atmospherical air were, by this means, reduced to no more than 26, but completely phlogisticated, I think it probable that some phlogisticated air was likewise formed at the same time. For, according to M. Lavoisier, atmospherical air consists of 27 parts of dephlogisticated, and 73 of phlogisticated air; whereas my result is in the proportion of 78 of phlogisticated to 22 of dephlogisticated air.

That 16 ounce measures of any dephlogisticated air that Dr. Woodhouse could make should be reduced to 4, and the remainder be *perfectly pure*, must be impossible; because to make dephlogisticated air perfectly pure, is hardly possible; and all the impurities in the 16 ounce measures would be found in the remaining 4. The Doctor should have said by what means he ascertained the purity of the air before and after the experiment. I hope he will repeat it with all the attention that it deserves.

M. Lavoisier, as Dr. Woodhouse informs us, page 467, says, that "if pure iron be melted in pure air, no fixed air "will be formed." But this is contrary to my uniform experience, with either dephlogisticated or atmospherical air. Dr. Woodhouse now found an ounce measure of fixed air in 90 grains of bar iron, so that here he must join me in pronouncing M. Lavoisier's experiment to have been very inaccurate, unless iron in France be different from that in England or America.

This fixed air, Dr. Woodhouse says, page 456, is formed by "a small quantity of *coal*, which all iron of commerce con-"tains:" but an ounce measure of fixed air, from 90 grains of iron, is no small quantity. If malleable iron contain so much *plumbago* as will give this large quantity of fixed air,

it ought to be found when diſſolved in an acid. But neither is any ſuch fixed air found in the inflammable air produced in that proceſs, nor when it is decompoſed by means of dephlogiſticated air; ſo that there is no pretence whatever for ſuppoſing that it is held in ſolution in that inflammable air. That malleable iron contains any conſiderable quantity of plumbago is utterly improbable. It is equally untrue that *precipitate per ſe*, or the *red precipitate*, contains fixed air. It never appears but in its decompoſition with inflammable air, when they unite to make it, as I think I have abundantly proved.

Dr. Woodhouſe ſays, page 471, that in five attempts to get fixed air from iron filings and red precipitate, he never ſucceeded. To this I can only ſay, that with me the experiment has never failed. But this is only one experiment out of many that prove the ſame thing.

He likewiſe ſays, page 472, that the fixed air which I produced by heating charcoal of copper in dephlogiſticated air, is no proof of its being formed from that kind of air, becauſe *carbone* is one of the component parts of alkohol, by means of which the charcoal of copper is made. But, ſurely, he did not recollect that the fixed air, procured in this manner, weighs conſiderably more than all the charcoal employed; in fact, between three and four times as much, (ſee the laſt edition of my *Obſervations on Air*, vol. iii. p. 377.) and that there is nothing elſe preſent into which the dephlogiſticated air can poſſibly enter: for of the charcoal nothing viſible is left.

With reſpect to this experiment, the French chemiſts give a much better explanation: for, they ſay that fixed air is carbone, diſſolved in dephlogiſticated air; and they make the proportion of each of the ingredients the ſame that I do. They only ſubſtitute their *carbone* in the place of phlogiſton.

Dr. Woodhouſe, after finding half an ounce meaſure of fixed air, in melting half a drachm of bar iron in 20 ounce meaſures of dephlogiſticated air, when 13 ounce meaſures of it were abſorbed, ſays, p. 467, "Certainly Dr. Prieſtley will "not ſay, that 13 ounce meaſures of dephlogiſticated air enter "into the compoſition of half an ounce meaſure of fixed air, "which muſt be the caſe if his theory be true." But Dr. Woodhouſe ſhould recollect, that, according to my theory, the greateſt part, if not the whole, of the *weight* of moſt kinds of air is water; and, therefore, when iron, by being converted into finery cinder, has got all the water from the 20 ounce meaſures of air, the oxygen contained in it may very well, for any

thing that appears, enter into the compoſition of half an ounce meaſure of fixed air; and one half the weight of this kind of air, I have clearly proved to be pure water.

I am, Gentlemen,
Your very humble ſervant,
J. PRIESTLEY.

Northumberland, July 24, 1799.

ARTICLE V.

Dr. Priestley's Reply to his Antiphlogistian Opponents.

No. III.

To the Editors of the Medical Repoſitory.

Gentlemen,

THIS letter will cloſe my reply to Dr. Woodhouſe, and I fear I ſhall not ſoon meet with another opponent ſo candid as he has ſhewn himſelf to be: I therefore hope that he will reſume the diſcuſſion ſo favourable to the diſcovery of truth.

1. Of the Calx of Mercury.

In order to invalidate my argument from a certain calx of mercury requiring to be heated in inflammable air, or in contact with ſome other ſubſtance ſuppoſed to contain phlogiſton, for its revival, Dr. Woodhouſe ſays, page 455, "It does not "follow that becauſe the ſulphate of mercury requires to be "deprived of its ſulphuric acid before running mercury can "be procured from it, that, therefore, all mercurial calces "require the addition of phlogiſton to be converted into mer-"cury."

But the Doctor does not ſeem to underſtand my argument, and, therefore, I ſhall ſtate it more diſtinctly. I find, no matter by what means, a certain calx of mercury, combined, I acknowledge, with ſome other ſubſtance; but ſince heat alone will not revive it, but heating it in inflammable air does revive it, while that air diſappears in the proceſs, it is natural to conclude, that ſomething contained in the inflammable air enters into the calx, and contributes to the revival of it, beſides ex-

pelling the other ſubſtance with which it was united; unleſs it can be made to appear that this inflammable air can be found united to ſomething elſe.

This is ſimilar to the caſe of finery cinder, a ſubſtance which, in my opinion, conſiſts of the calx of iron united to water, which cannot be expelled but by the introduction of phlogiſton to ſupply its place. In this caſe the antiphlogiſtians ſay that the inflammable air which diſappears aſſiſts in forming *water*, an opinion which, I think, I have ſufficiently refuted. I now aſk, what becomes of it in the revival of this calx of mercury, in which no water is found? I therefore ſay, that if inflammable air, or the phlogiſton contained in it, be a neceſſary component part of this particular piece of iron, or of this particular portion of mercury, it muſt be a neceſſary ingredient to the conſtitution of iron and mercury in general.

2. Of Finery Cinder.

Dr. Woodhouſe confirms my experiment of the abundant production of inflammable air from heating dry finery with dry charcoal; but he ſays, p. 465, the iron is not revived. I muſt, however, take the liberty to ſay, from my former experiments, that the iron is completely revived in this proceſs; and I made the experiment too often to be miſtaken. On this ſubject I ſay, *Obſervations on Air*, vol. vi. p. 375, "This (i. e. the re-"vival of iron from finery cinder) is effected in the moſt "complete manner by heating it with a burning lens in in-"flammable air. But it is likewiſe done by cementation with "charcoal, coak from pit coal, or raw coal. In all theſe "proceſſes the finery cinder loſes about one third of its weight, "and is then perfectly ſoluble in acids, and attracted by the "magnet. Conſequently it is perfect iron. Iron thus made "is partially malleable."

Dr. Woodhouſe ſays (Med. Rep. vol. ii. p. 401), that I ought to have remembered that this ſubſtance cannot be revived in one of Lewis's ſmall black furnaces. But where does it appear that I ever knew what he expects me to remember. If this furnace will not anſwer the purpoſe, let more heat be applied. My brother-in-law, Mr. Wilkinſon, has converted many tons of finery cinder into iron in his ſmelting furnaces.

He ſays that my explanations of this experiment will not account for the formation of the fixed air which is formed at

the ſame time. But I maintain that charcoal itſelf contains all the elements of fixed air, ſince it is produced from it when it is heated in ſteam: and it is ſurely ſufficient, if any hypotheſis be conſiſtent with itſelf, though not with concluſions from other theories.

If Dr. Woodhouſe will look into my account of the analyſis of different kinds of inflammable air, he will find that the fixed air procured from the decompoſition of that which is procured in this proceſs, muſt be compoſed, in part, of the pure air in which it is fired; becauſe the weight of this fixed air exceeds that of the inflammable air that is employed.

This is the "carbonated hydrogen gas which," Dr. Woodhouſe ſays, page 465, "is formed by hydrogen of the water "in the finery cinder diſſolving part of the coal; while its oxy-"gene unites with another part of the coal, and makes fixed "air." This ſame coal, or *carbone*, I cannot help obſerving, is a wonderfully convenient ſubſtance, forming whatever the theory requires that it ſhould do. But to ſuppoſe, as Dr. Woodhouſe does, that finery cinder contains water, and ſo much of it as is neceſſary to form all this air, both inflammable and fixed, is to abandon the moſt fundamental principle of the new theory, which requires water to be decompoſed in paſſing over hot iron, the oxygene alone remaining in the iron, and the hydrogen eſcaping in the form of inflammable air. And it is only by comparing the addition of weight to the iron that the proportion of the oxygene and the hydrogen in water is aſcertained. Whereas, according to Dr. Woodhouſe, there muſt be nothing, or next to nothing but mere water in the finery cinder, which accords with the doctrine of phlogiſton, and no other. Beſides, how can that which is attached to the iron be water, when its hydrogen has been ſeparated from it.

Thus does Dr. Woodhouſe take from the new theory one of its neceſſary ſupports, viz. the *decompoſition* of water. There only remains the argument from its *compoſition*, by burning inflammable in dephlogiſticated air. But when an hypotheſis ſtands only on *two* legs, and one of them is taken away, it muſt be left very lame.

3. Of the Formation of Nitrous Acid.

Dr. Woodhouſe, not finding more than three grains of nitrous acid from the decompoſition of 56 ounce meaſures of

dephlogiſticated air, ſays, p. 474, "the theory of Dr. Prieſtley muſt certainly be wrong; for it is not probable that 56 ounce meaſures of dephlogiſticated air ſhould enter into the compoſition of three grains of nitrous acid." But the acid thus formed is ſo highly phlogiſticated, and diffuſed through ſo large a ſpace, that it is no wonder at all that no more of it ſhould be collected. In this ſtate water retains it very imperfectly. But that even three grains, or one grain, of the acid ſhould be formed from the exceedingly ſmall quantity of phlogiſticated air which could not be excluded in ſome of my experiments, is abſolutely impoſſible. In ſome of them, I am confident, it was an hundredth, or a thouſandth part of a grain in weight, when the acid I collected was ſeveral grains.

Let any perſon attend to the laborious experiments of Mr. Cavendiſh, on the decompoſition of a ſmall quantity of phlogiſticated air, and ſay whether it be credible that ſo much nitrous acid could be produced from any quantity of phlogiſticated air by a ſingle exploſion in this way, when it required thouſands of exploſions in his.

I think I have noticed every thing of much conſequence in Dr. Woodhouſe's publication. Somewhere, however, he ſays, that manganeſe was not revived, though ſome inflammable air in which it was heated diſappeared. But ſeveral calces imbibe much inflammable air, or phlogiſton, before they are completely revived; and the metal of manganeſe is not procured without great difficulty in any proceſs.

With thanks for your continued indulgence,

I am, Gentlemen,

Your very humble ſervant,

J. PRIESTLEY.

Northumberland, July 26, 1799.

☞ The reader will be pleaſed to correct the following ERRATUM, juſt diſcovered, in a former communication of Dr. Prieſtley's.

Vol. ii. p. 265, line 10 from the bottom, for "the" read *though*.

ARTICLE VI.

Obſervations on the YELLOW FEVER, *as it appeared at Wilmington (Delaware) in the Summer and Autumn of* 1798: *Communicated in a Letter from* JAMES TILTON, *M. D. Preſident of the Medical Society of Delaware, to Dr.* MILLER.

Wilmington, (Delaware) June 18, 1799.

Dear Sir,

IN contemplating ſo dreadful a calamity as the yellow fever, it ſeems to be incumbent on every man to contribute his mite for the protection of ſociety againſt its ravages. The correct information received through the medium of the Medical Repoſitory has ſuggeſted, that in return for the uſeful hints and obſervations from others, we ought to give ſome account of this peſtilential epidemic, as it appeared among us.

The manner in which I am obliged to diſſipate my time, in ſubſerviency to a ſhattered ſtate of health, has hitherto prevented me from writing to you on this ſubject. I hoped too, that ſome report of our committee of health might have ſaved me the trouble; but in this I have been diſappointed.

As the ſeaſon is now approaching, when our country will again run the riſk of this ſcourge of mankind, I have haſtily thrown together ſome thoughts on this ſubject, to be diſpoſed of as you ſhall think proper. Without dwelling long on the hiſtory of the diſorder, or waſting time in fine-ſpun theories, I ſhall confine myſelf to ſome practical obſervations, which, I apprehend, have not been duly regarded by others.

At Wilmington we have no apprehenſion of domeſtic origin. Every medical character, in this place, takes it for granted, that the diſeaſe was imported from Philadelphia, and no otherwiſe created. It appeared to me alſo, that infected houſehold goods and furniture, brought from the city, by our ſhallops, had more influence in ſpreading the contagion, than diſeaſed perſons: for it was very remarkable, that the diſeaſe was not communicated from the firſt perſon who died of it, and who came down and ſickened in the land ſtage. But, when the fever became epidemic, it took riſe at the water's edge, and infected all, or with few exceptions, gradually

up to High-ſtreet. Above this, the town is more thinly built, and the caſes were ſolitary as in the country.

Although the diſeaſe made its appearance among us, in the beginning of Auguſt, we hoped for our uſual exemption, until near the middle of the ſame month, when the epidemic nature of it appeared ſo manifeſt that a committee of health were appointed, and a ſlight hoſpital arrangement was conſtructed, for the relief of the poor and neceſſitous. Soon afterwards the principal phyſicians of the borough were called upon for their advice; and, with a view of obtaining the greater maſs of information, the committee requeſted them to report individually, rather than collectively. By this time, however, my brethren were ſo diſordered, in their perſons or families, that they could do little more than make an apology for their non-compliance. With great difficulty, under the moſt diſtreſsful interruptions, I found means of preſenting to the committee the following

"REPORT.

" Gentlemen,

" THE cauſe of humanity operates ſtrongly upon my mind, in concert with the reſpect due to the Committee of Health for the borough of Wilmington, to make the beſt Report in my power, relative to the moſt expedient and uſeful hoſpital arrangements, for the accommodation of ſuch unfortunate perſons, whether foreigners or native citizens, as may ſtand in need of an aſylum during the preſent contagious epidemic. I wiſh, indeed, that the ideas now ſuggeſted, may be worthy of regard, not only at the preſent time, but that they may ſerve as a baſis to build upon, on all future occaſions, which, in the events of providence, may require ſimilar proviſions.

" It might be expected, that one of the firſt objects in a Report of this ſort, would be relative to the excluſion of contagion from among us. How far the exiſting laws put it in our power to take meaſures of this ſort, will deſerve the deliberations of the committee; and the phyſicians will be always ready to aſſiſt them, with all the information they poſſeſs relative to this point. But regarding the Yellow Fever as already amongſt us, we ſuppoſe it more immediately neceſſary to report a convenient hoſpital arrangement; and this is as much as can be accompliſhed within the time to which we are limited.

" The firſt point I ſhall ſuggeſt to your conſideration is the moſt proper ſituation for an hoſpital. Where it now ſtands, in my apprehenſion, is the moſt improper place that

could be chosen. On the southern side of a hill, rising for a great extent into a mountain, the heat of the air must be increased; and the south-westerly winds, which prevail during our hottest weather, must waft the poisonous effluvia, exalted in their malignity, over the town. A situation northward or eastward of the borough would be much preferable. The prevailing south-west wind would then carry every species of exhalation, arising from the hospital, in a direction from the town; and the cooler breezes from the north and east, when they happened, would be without hazard, as the exhalations would be little or none at all, and in no degree exalted as by the scorching south-west. If, moreover, a situation could be obtained, where all the offal of the hospital could be emptied into the waters of the Christiana or Brandywine, it would contribute greatly to cleanliness; and the course of those rivers would give a refreshing current to the air, which cannot be expected in the present situation of our hospital, under cover of a mountain.

"The next article to which I shall call your attention is the importance of earthen or ground floors, in all cases where contagion is to be suspected. This point I regard as fundamental and essential. You will, therefore, bear with me, while I dilate somewhat upon this subject: and when you reflect that I have served seven years in contagious hospitals; that during the whole of that period it was never my lot to be a mere parade officer; that I always stood at the post of duty, which was also the place of observation;—you may reasonably suppose I ought to have some confidence in the opinions and advice which, at your request, I now submit to your discretion.

"The grave-yards of this place may call to your recollection the destructive nature of the contagion which existed in the hospitals of the American army. This used often to be the subject of conversation, as the Yellow Fever now is. In a discourse which I had with a New-England officer, respectable for his age and former services, he mentioned, that the celebrated general, Count Saxe, had noted in his Memoirs, or Reveries, that ground floors were much more salubrious in military hospitals than those made of wood or plank. The observation struck me with great force. It immediately occurred to me, that we purified clothing and furniture from infection of every kind, by burying them in the earth; and that putridity, so commonly connected with the idea of contagion, was, in like manner, abstracted from all substances whatsoever.

From this time forward, I was led to remark the great ſucceſs of tents, in compariſon of churches, barns, &c. when employed as hoſpitals. A houſe, with a plank floor, never failed to become contagious in a ſhort time; ſo that men who entered the hoſpitals for trivial complaints, often died of the contagion. I need ſcarcely obſerve, that diſorders, bad of themſelves, were aggravated by ſuch contagious places of reſidence. In the winter, however, it was impoſſible to employ tents. I, therefore, in the hard winter of 1779 and 1780, while the army was cantoned on the mountains near Morriſtown, with much difficulty, found means of building an hoſpital at Baſkingridge, with no other flooring than the common ſurface of the ground; without a chimney, the fire was made in the middle of the ward, and the ſmoke paſſed through a funnel in the middle of the roof. My ſucceſs was equal to my moſt ſanguine expectation. This houſe was free from contagion, while the barns and out-houſes of the neighbouring farms, though leſs crowded, manifeſted the uſual misfortune. Dr. Ruſh, in one of his Eſſays,* takes notice of the ſucceſs of this hoſpital. As well as I recollect, he adverts principally to its advantageous mode of ventilation. But, I am perſuaded, it could not have eſcaped the contagion, if the floor had been of wood. Long details might be added to this ſhort hiſtory, to eſtabliſh the advantages of ground floors.

"But if a caviller at this doctrine ſhould contend, that the contagion of camp fever, dyſentery, &c. the uſual attendants upon armies, is ſpecifically different from that of Yellow Fever, I anſwer, it matters not; becauſe, in each and every caſe, a ſhip and wooden houſe are moſt noxious devices for imbibing and exalting the contagion; while, in every caſe, likewiſe, we bury clothing and furniture in the ground, as the moſt effectual means of purification.

"If theſe premiſes are admitted, the arrangement I ſhall now propoſe, follows of courſe, viz.

"That the hoſpital ſhall be eſtabliſhed on the northern or eaſtern ſide of the borough, on the Chriſtiana or Brandywine, as ſhall be found moſt expedient and convenient.

"That, inſtead of a houſe, TENTS, of the ſize of the ordinary horſeman's tent, ſhall be provided for the ſick; one to be appropriated to every patient. The horſeman's tent opens advantageouſly on every ſide, for the admiſſion of air. Every

* An Account of the Bilious Yellow Fever in 1794.

tent that has been once uſed, ſhould be thrown down, and ſoaked in water, before it is again employed. Thus every patient will be freed from all the dangers proceeding from his fellow ſufferers, as well as from himſelf.

"That no houſe will be neceſſary, but a kitchen to cook in, unleſs our misfortunes ſhould be extended to the cold weather; and then a ſlight building of any ſort, with a ground floor, a funnel in the middle of the roof, and no chimney, after the example of Baſkingridge hoſpital, for the reception of patients in a ſtate of great frailty, at night only, might ſuffice. For permanent uſe, it would be better to have the walls plaſtered, ſo as to be occaſionally white-waſhed. When the weather permits, a little fire might be kindled in the middle of the floor; and the patients lying round on every ſide, with their heads to the wall, would have their feet towards the fire. The ſmoke circulating through the upper parts of the ward would purify the air, and paſs off through the funnel, without inconvenience to the ſick. Every ſide of the building ſhould be capable of opening widely, and cloſing again, as the ſun, wind and rain may require. The ground floor will, above all things, protect from contagion. When any change or renovation ſhall be required in this, the old ſurface may be ſcraped off, or a new one put on.

"Upon this plan it is alſo manifeſt that the expenſes may, from day to day, be readily accommodated to the exigencies of the occaſion; a difficulty that has been lamented in other places.*

"In ſhort, the frugality and practicability of this meaſure are equally apparent; and I can aſſure the committee, that much experience ſatisfies my mind of its great utility. Who would not, indeed, rather truſt himſelf in a tent, where the friendly winds waft away the poiſonous exhalations, and our kind mother earth lends her aſſiſtance in abſorbing every noxious diſcharge from the body, than in a confined room, where the air we breathe muſt be charged with contaminating poiſon, and the plank floor, reverberating, as it were, the effluvia of the patient upon his own body, muſt operate as a perpetual ſource of injury?

"Could the doctrine I have endeavoured to eſtabliſh be put to the proof, and the public mind ſatisfied of its truth and validity, inſtead of that reluctance to go to the hoſpital which

* At Baltimore particularly.

we now obſerve, every man of common ſenſe would fly to it as to a city of refuge. Phyſicians and nurſes would attend with confidence; and it would be eaſy to baniſh from our towns any contagious ſickneſs.

" It may not be impertinent to obſerve, that when imperious circumſtances require thoſe who are well to leave their habitations, they might, as well as the ſick, take advantage of tents, by forming an encampment near their ordinary place of reſidence. This would certainly be better than to fly, in terror, over the whole face of the country. The Governor of Pennſylvania has, on a former occaſion, ſet the example, and, I ſhould ſuppoſe, others might profit by doing ſo likewiſe.

" Theſe, Gentlemen, in my opinion, are the great outlines which demand your immediate attention. Secondary objects, ſuch as bedding, furniture, the internal government and management of the hoſpital, may be further regarded in due time.

" With great reſpect,
" I am, Gentlemen,
" Your moſt obedient ſervant,
" JAMES TILTON.

" COMMITTEE *of* HEALTH *for the* Borough *of* Wilmington.
" *Wilmington, September* 6, 1798."

In this report, you will perceive, I have not attended to the critical diſtinction between *contagion* and *infection*, but have rather conformed to the prevailing impreſſion at the time; for every body, at Wilmington, ſuppoſed the diſeaſe to be *ſpecifically contagious*. The highly offenſive ſmell proceeding from the patients, ſo as to characterize the diſeaſe, and often to affect the ordinary ſenſe of ſmelling, at a much greater diſtance* than that to which the contagion is limited by Dr. Chiſholm, contributed not a little, with the rapid ſpread of the fever, to give this general impreſſion.

At the ſame time that, in this fever, all our ſenſes are ſtrongly impreſſed with the idea of contagion, proceeding immediately from one perſon to another, it muſt be confeſſed, it has ſome characteriſtics in common with infectious diſor-

* Ten feet.

ders. Like the jail fever, dyſentery, &c. the contaminating poiſon of the Yellow Fever may be exalted through all the grades of malignity, by external circumſtances, free from all connection with diſeaſed bodies. Hence it is not ſurprizing, that phyſicians of the greateſt eminence ſhould deem this fever both contagious and infectious. I mention theſe things, not with deſign to enter into controverſy, but to ſhew that, in each and every caſe, whether contagious, or infectious, or both, the practical obſervations, which I have to offer, are equally applicable.

If the contaminating poiſon of Yellow Fever adheres as tenaciouſly to wood as to cotton or wool; and, by a permanent connection of this ſort, it becomes more and more exalted in its malignity; if the common earth manifeſts a capacity for attracting this kind of poiſon, abſtracting it from other ſubſtances, and neutralizing or correcting it, ſo as to render it totally inoffenſive; it would require more than a ſhort eſſay to give a full view of the extenſive application of this principle in medical practice.

When the Yellow Fever rages, the principles which I have adopted would direct every human creature, both ſick and well (a neceſſary watch or guard excepted), to quit the infected city or town, and encamp on the open plains. The ſick ſhould carefully avoid plank floors to their tents, and lie upon cots or bunks, with their legs ſtanding upon a freſh ſurface of the ground, or a graſs ſod. The lazarettos, now ſo generally in contemplation, ſhould conſiſt chiefly of a well choſen, high, dry, and extenſive plain, and a plenty of horſemen's tents; a rivulet running through or by it would be of great advantage. Such houſes as might be deemed neceſſary, ſhould be built as deſtitute of wood as poſſible; and wards, deſtined for the ſick, ſhould have ground floors, with a fire place in the midſt, and be ventilated through a funnel in the middle of the roof, as deſcribed in the foregoing report. When our ſhips or veſſels, with their crews, become infected, they ſhould make the firſt port, and encamp all hands on ſhore, until the men are recovered, and the veſſel thoroughly cleanſed. When, by any misfortune, the infection ſhall have crept into any of our trading towns, it may be expedient to cover a ſmall part of the ſtreets, where the infection firſt manifeſts itſelf, with freſh earth, rather than ſcrape the pavements bare, by which means the heat of the atmoſphere is exalted to intemperature, and all abſorption prevented. Might not patients employ the

ground bath to advantage? And will not a multitude of advantages occur to every practitioner, as ſoon as the principles I have laid down ſhall be eſtabliſhed by common conſent?

Science contributes not a little to illuſtrate our ſubject. The contagious matter of the plague is deemed heavier than the common atmoſphere; and, therefore, has a conſtant tendency to the earth. The poiſon of Yellow Fever, upon due inveſtigation, would probably be found to poſſeſs the ſame quality. We can readily conceive, too, of an elective attraction between the conſtituent principles of common earth and the gaſeous poiſon of yellow fever, whereby a *tertium quid* may be produced that is totally inoffenſive. In no caſe, perhaps, would Profeſſor Mitchill's doctrine of *ſepton* more advantageouſly apply. But I am not ambitious of multiplying words. My object is only to ſketch an outline, which, if well received, will ſoon be filled up with the requiſite comments, by men of more talents and more leiſure than are at my diſpoſal.

I will only add, that the little hoſpital of this place ſerves as a pointed evidence in proof of the doctrine advanced. The board of health, regardleſs of my advice, neglected to remove their hoſpital, and admitted the firſt patient into a ſmall houſe they had provided, floored with plank, and built totally of wood. It ſoon became apparent, that every patient placed in this houſe died of courſe. A child of five years old, whoſe father and mother had died of the fever, was put into one of the tents, becauſe it had no other place of accommodation. This child, inadvertently ſtrolling to the wooden houſe, was ſeized with a tremulous agitation of the whole body; a violent fever inſtantly ſucceeded, and the child died in three or four days. So manifeſt was the noxious influence of this wooden building, that the patients were induced to deſert it utterly, and remove their ſafer tents to the utmoſt diſtance permitted by the limits to which they were confined. The nurſes, and other attendants on the ſick, who were quartered in tents, all eſcaped the infection.

I am, dear Sir,

Your friend and humble ſervant,

JAMES TILTON.

ARTICLE VII.

Remarks on the EPIDEMIC *of the Summer and Autumn of* 1798, *at Wilmington: Communicated in a Letter from* GEORGE MONRO, *M. D. to Dr.* MILLER.

Wilmington (Delaware), June 20, 1799.

Dear Sir,

THERE is nothing I can afford, from this place, reſpecting the epidemic of laſt ſeaſon, differing in ſubſtance from more important communications already publiſhed in your very uſeful Repoſitory. But I ſhall, for the ſake of general information, agreeably to your requeſt, endeavour to furniſh you with a few remarks reſpecting the Yellow Fever, as it occurred here the laſt ſeaſon.

The firſt caſe of this diſeaſe happened in the beginning of Auguſt. The infection was evidently traced to a diſeaſed veſſel in Philadelphia, on board of which the perſon had been, as he was about renting a houſe in that part of the city where this veſſel lay, and where the fever firſt and principally prevailed. He ſickened on the road, coming home, grew very ill immediately on his return, and died in a few days. None of the family afterwards ſuffered from his illneſs.

Very ſoon after this, all that part of the town connected with the wharves became diſeaſed. The upper and high parts, where the houſes were thinly built, ſuffered very little. Notwithſtanding the inhabitants very generally went into the country, the fever continued to increaſe in its ravages among the remaining citizens, till it received a check by the operation of cool weather. The laſt caſe I recollect happened on the 6th of November.

In this fever, all the forms mentioned by various writers appeared, viz. from the loweſt grade of febrile affection to the moſt violent and inflammatory. One inſtance occurred of a child dying with the ſtrongeſt ſymptoms of convulſive ſpaſm. The practitioners of eſtabliſhed reputation here unanimouſly agreed upon the beneficial effects of mercury. Blood-letting was frequently required; but a free evacuation of the firſt paſſages, followed by mercury, was moſt univerſally uſeful. I have ſeen ſeveral inſtances where ten grains of calomel, and fifteen of jalap, ſeemed to carry off all the

ſymptoms, when given upon the commencement of attack. Where the mouth could be made ſore with mercury by the third or fourth day, I do not recollect any inſtance of mortality, unleſs in one caſe, where the patient had before ſuffered from a long continuance of ſyphilitic affection.

The great mortality experienced here principally aroſe, in my opinion, from the neglect of proper remedies. An unhappy prejudice was generally diffuſed, by ſome French characters, againſt the uſe of mercury. The inhabitants conceiving, I ſuppoſe, that ſuch perſons muſt neceſſarily be more converſant with this novel affliction than their own phyſicians, were, of courſe, more diſpoſed to reſpect their counſel. One of theſe gentlemen, bearing the name of a ſurgeon, was entruſted with the care of the ſick who were removed to the ſuburbs of the town; and, as his treatment of their complaints was chiefly confined to gentle laxatives, manna, I believe, and barley-water, you may readily judge how incompetent theſe would be found to encounter ſo formidable a diſeaſe.

Great benefit was obtained from alkaline remedies in relieving the diſtreſſing cardialgia, which, in ſo many inſtances, was the fore-runner of black-vomiting. But where free evacuations, in the firſt inſtance, were neglected, even with a liberal uſe of mercury afterwards, little good could be expected from any medicine.

One inſtance of pregnancy, as far advanced as the ſixth month, with conſiderable determination to the head, as evidenced by great inflammation of the eyes, ſtupor, &c. recovered. The patient was blooded ſeveral times, and highly ſalivated on the fourth day from the attack. This caſe was peculiar, from vomiting a large quantity of very dark coloured matter in the early ſtage of the diſeaſe, which was ſucceeded by profuſe ſweating. The diſcharge from the bowels was very inconſiderable. Would not ſuch inſtances very much encourage the uſe of full vomiting, in the firſt approach of this dreadful enemy?

Several caſes of convaleſcency put on dyſenteric ſymptoms at the cloſe of the ſickly ſeaſon.

Accept this ſhort ſketch; and believe me to be, with beſt wiſhes for the ſucceſsful continuance of your very laudable and highly important undertaking,

Moſt reſpectfully yours,

G. MONRO.

ARTICLE VIII.

A Letter from JOHN MACLEAN, *M. D. Profeſſor of Mathematics and Natural Philoſophy in the College of New-Jerſey, in Anſwer to one addreſſed to him by* JAMES WOODHOUSE, *M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania.*

College of New-Jerſey, June 15, 1799.

SIR,

THE fourth number of the ſecond volume of the Medical Repoſitory has been ſent to me, and I am happy that you have called my attention to ſeveral important points in chemiſtry, on which you believe my aſſertions are abſolutely erroneous.

In my examination of Dr. Prieſtley's conſiderations on the doctrine of phlogiſton and the decompoſition of water, I made free with his opinions, becauſe I conſidered them as unwarranted and contradictory; and I condemned many of his experiments, becauſe I was ſatisfied they were inaccurate, and that as well from my own experience, as from comparing them with others, made by himſelf and the French chemiſts.

It ſeems, however, that for ſeveral particulars you are inclined to think I had no right to do ſo; and you tell me I am not yet "the conqueror of this veteran in philoſophy." Now, it is to remove the unfavourable impreſſion which my obſervations have made upon you, that I uſe the freedom to trouble you with this anſwer, and I hope you will not refuſe your attention to it.

In the firſt place, ſuffer me to aſſure you, I am far from pretending to be ſuch a conqueror. Although I could have related many experiments of my own, yet, as they were chiefly repetitions of what had been done before, I gave the original authorities; ſo that it is plain, had I ſuppoſed any conqueſt was to be gained, or any honour to be reaped, I was determined it ſhould be by thoſe who were moſt juſtly entitled to it. Indeed, I am, on all occaſions, ready to acknowledge my inability to cope with Dr. Prieſtley. He can write faſter than I can think, and experiment faſter than I can write. But although I be deficient, it may not be ſo

with others. You are ſaid to have written an anſwer to the Doctor, and who knows that the important conqueſt may not be in reſerve for you?

The firſt particular you allege againſt me is the having agreed with the French chemiſts, that turbith mineral is an oxyd of mercury; and the having aſſerted, that any ſubſtance into which its mercury may be converted by a red heat, does not require any addition to conſtitute it a metal: and, after having told me the contrary of this is true, you mention ſeveral circumſtances, which prove that the turbith is a ſulphate of mercury.

Be ſo obliging as to look over the eleventh page of my lectures; you will there find a reference to the 305th page of the tenth volume of the *Annales de Chimie*. It was publiſhed in the year 1791; but as it is poſſible you may not have had an opportunity of conſulting that work, permit me to preſent you with the following extract:

"Quelques chimiſtes ont penſé," ſays Mr. Fourcroy, "que le turbith minéral n'étoit qu'un oxide de mercure, et "qu'il ne contenoit pas d'acide ſulfurique. Mais l'opinion de "Rouelle ſur cette matière eſt confirmée par mes experiences. "Car en traitant le turbith le plus lavé et le mieux deſſéché, "par l'acide muriatique, la diſſolution precipite du ſulfate de "baryte par le muriate de baryte, à la vérité, beaucoup "moins que le ſulfate de mercure neutre dont nous avons "parlé plus haut. Je l'appellerai done *ſulfate de mercure "avec exces d'oxide, ou ſulfate de mercure jaune.*"

From this it appears, Mr. Fourcroy, one of thoſe who are uſually called the French chemiſts, had aſcertained, about eight years ago, that turbith mineral is really a ſulphate of mercury, and, in conſequence, had given it an appropriate name.

Now, take the trouble to turn to the twelfth page of my lectures. It is there ſaid, expreſſly, the ſubſtance obtained from turbith mineral by heating it, is different from an oxyd of mercury. To be ſure, I gave a quotation from Fourcroy's Elements, in which the turbith was called an oxyd; but the reference to the Annales de Chimie, which were publiſhed long after the Elements, and my declaration, were ſubſequent to the reading of that quotation; and, conſidering the ſubject then in queſtion, it was not neceſſary to be more explicit. Beſides, the lectures you have ſeen were merely ſupplementary; and I had, on a former occaſion, told my claſs, that turbith

was compoſed of an oxyd of mercury, and ſulphuric acid. You were miſtaken, then, in imputing, either to the French chemiſts or to me, the opinion, that turbith is a ſimple oxyd of mercury.

In the firſt experiment I made on the effect of a high temperature, on turbith mineral, about two drams of it were put into a glaſs tube, ſhut at one end; and this being placed among burning charcoal, the other was thruſt under a jar filled with and inverted into mercury. The turbith ſoon became of a deep red colour; oxygen and ſulphurous acid gaſes paſſed into the jar; and running mercury, with a white ſubſtance cloſe by it, collected in the tube. On the tube being removed, that part of it where the turbith had been put was found empty.

Being puſhed a little farther into the fire, the white ſubſtance became yellow, and afterwards red; it then melted and boiled; the mercury removed to another part of the tube; oxygen and ſulphurous acid gaſes eſcaped; and, on the red ſubſtance being diſſipated, the tube was again removed, and a little of a white ſubſtance was found cloſe by the mercury, though, as before, nearer the fire.

The tube was puſhed into the fire for ſeveral times ſucceſſively, and always with a like effect; the mercury increaſing, and the white ſubſtance diminiſhing in quantity, until, at laſt, it could not exceed the fourth part of a grain; and then, on account of the ſhortneſs of the tube, the operation was diſcontinued.

It was from the reſult of this experiment that I informed my pupils, about four years ago, as may be ſeen from the notes now in poſſeſſion of ſome of them, that turbith mineral might be entirely reſolved into oxygen gas, ſulphurous acid, and running mercury. It was from the ſame experiment I agreed with the French chemiſts, and concluded, that the mercury in turbith mineral, or any ſubſtance into which it may be converted by a red heat, does not, to uſe Dr. Prieſtley's phraſe, require any addition to conſtitute it a metal. And, having tried it over and over again, I do, with the moſt perfect confidence, inſiſt, you were wholly wrong in telling me the contrary of my aſſertions was true.

You proceed to remove what you ſuppoſed might be alleged againſt turbith mineral being a ſulphate of mercury; and you ſay, when it is expoſed to a red heat, "the ſul-" phuric acid leaves one part of it, and joins to another,

" which ſublimes in the form of a white ſalt. That part " which the acid deſerts is converted into an oxyd, is revived " without addition, and yields pure air." Next you inform me, the white ſalt, or ſulphate, is the calx which Dr. Prieſtley ſuppoſes incapable of reduction without addition; and that it is ſometimes mixed with cinnabar, and hence has a red colour. And you then remark, I ſhould have aſcertained all this had I intended to acquire the character of an accurate inveſtigator.

My lectures were not written with a view to acquire ſuch a character: it is more than probable I do not deſerve it. But, from the obſervations which I have made, I cannot help thinking, there is more of opinion than of fact in what you have ſaid.

It is true, when turbith mineral is heated, a quantity of white ſulphate of mercury is obtained; but it is not ſo certain that this ſulphate is the ſubſtance alluded to by Dr. Prieſtley, nor that the red colour, which he ſays it always has, is owing to an admixture with cinnabar.

A quantity of the white ſubſtance obtained in the experiment with the turbith being collected, the part of it which had been neareſt the fire was ſlightly yellow, while that which had been fartheſt from it was white.

Some of it, being thrown into warm diſtilled water, afforded turbith; and the fluid, being filtered, yielded a precipitate on the addition of pure potaſh.

Another portion being put into a glaſs tube, ſhut at one end, and placed on live coals, became yellow, then red, and melted. The tube being withdrawn, it changed to a ſolid, which was at firſt yellow, but which gradually turned nearly white. Being ſet again on the fire, the colour altered as at firſt. The whole melted, boiled, and diſengaged ſulphurous acid and oxygen gaſes; then ſome mercury appeared in the tube; a little ſulphurous acid was again emitted; and a white ſubſtance, and cloſe by it a red coloured fluid, collected in the tube: at length the whole of the ſubſtance put into the tube was diſſipated.

On the tube being removed, the red fluid turned ſolid, and was at firſt yellow, but afterwards almoſt white. The mercury, being ſcraped with a glaſs rod, was ſhaken out; and the white ſubſtance, being puſhed to the bottom of the tube, was heated, and afforded the ſame products as at firſt. And

this operation being repeated ſeven or eight times, the whole was reſolved into mercury, and the gaſes above-mentioned.

After conſidering all theſe phenomena, I concluded,

1. The white ſubſtance from the turbith was a ſulphate of mercury, and of the ſame ſort as may be got by boiling ſulphuric acid with mercury, and heating the mixture till the ſuperfluous acid is driven away.

2. Its red colour was dependent on the temperature to which it was expoſed.

3. It was formed in conſequence of ſulphuric acid and mercury diſengaged from the turbith, and meeting, when in vapour, at a temperature favourable for their action on each other.

4. It was depoſited on a part of the tube where the temperature was not ſufficient to decompoſe it.

5. The ſulphurous acid gas which firſt appeared, was owing to the decompoſition of ſome of the ſulphuric acid in the ſalt.

6. The oxygen gas and mercury proceeded from the decompoſition of the oxyd of mercury: and,

7. The ſecond portion of the ſulphurous acid gas followed on the action of the ſulphuric acid, and the mercury when in vapour.

I am the more inclined to think, the ſulphate was formed in the manner juſt mentioned, from its being ſo eaſily decompoſed. If it had ſublimed ready formed from the turbith, it ought, when expoſed to the ſame temperature, to have riſen again without alteration.

It does not ſeem at all probable, that the ſulphate of mercury, either pure or coloured with cinnabar, is the calx which Dr. Prieſtley alludes to. He ſays, in his remarks on my obſervations, that it is a red friable ſubſtance, which the rays of the ſun, concentrated by a burning lens of ſixteen inches in diameter, converts into a yellowiſh glaſs, with the loſs of about three tenths of its weight. Now, if it was the ſulphate of mercury, it could be eaſily decompoſed by encreaſing its temperature; and if it was cinnabar, every part of it could be raiſed into vapour: but in neither caſe would it yield a yellowiſh glaſs. Nay, I can ſcarcely believe the ſulphate obtained by heating the turbith is at any time mixed with cinnabar. The red colour which it had in my experiments was not permanent; and, ſince the reading of your letter, I

have, at different times, mixed different proportions of charcoal with turbith, and heated them, but I got no cinnabar. Indeed, the quantity of oxygen in turbith, exclusive of that in its sulphuric acid, is so great, that it must be more than sufficient to consume any inflammable substance, which could, accidentally, and unobserved by any but a bungling chemist, fall into it.

I will not indulge in conjecture, about what may have been the cause of Dr. Priestley's want of success: but this much I will undertake to ensure—if he uses turbith, equally pure with that which I employed, he will, with due pains, reduce the mercury in it without addition.

My turbith was prepared from mercury, previously distilled in a glass retort, and sulphuric acid, first distilled, and afterwards boiled, in glass vessels.

The mercury, with twice its weight of the acid, was put into a glass matrass, and set on a charcoal fire. When the resulting sulphate had become dry, and some of it slightly yellow, the matrass was removed and broken. The sulphate, which was found to be perfectly free from mercurial globules, after being reduced to powder in a glass mortar, was thrown into six times its weight of warm distilled water, and boiled for about ten minutes. On being suffered to cool, a beautiful yellow powder precipitated to the bottom of the vessel. The supernatant liquor being poured off, as much distilled water was added, and the mixture treated as before. This washing, with the distilled water, was thus continued, until some of the fluid, being cooled, and passed through paper, was almost insipid, and afforded only a slight precipitate, on the addition of a solution of muriated barytes. Then, as much as possible of the liquor was poured off, and the remaining water was exhaled by a gentle heat.

I suppose there can be no objection to turbith prepared in this manner; and that it ought to be considered as perfectly pure.

Some address is necessary in the experiments on the decomposition of the turbith and white sulphate. If it be performed in a glass tube, the coals may be set in a common chafing-dish; but they must surround, yet not touch, the part of the tube containing either of the substances. When they do not surround it, the heat is not sufficient; and when they touch it, the glass is apt to melt.

If the fire be too low, it may be enlivened by blowing with the mouth.

It ſometimes happens, particularly with the white ſulphate, and when the glaſs contains much alkali in its compoſition, that the inſide of the tube is corroded; ſo that it may be proper to uſe freſh tubes for the ſucceſſive operations to which it is neceſſary to ſubject the ſulphate.

When the glaſs is much corroded, a white cruſt adheres to its inſide; and, if the heat has been ſufficient, and long enough continued, it will be found to conſiſt chiefly of ſiliceous earth.

If the quantity of the ſulphates be great, it will be neceſſary to uſe an earthen retort, for thoſe of glaſs are ſo thin, that they melt before the decompoſition is finiſhed.

You next charge me with the having aſſerted that the red oxyd of lead contains more oxygen than the black oxyd of iron, and with the having ſuppoſed that it is from that circumſtance the firſt oxygenates the muriatic acid, while the latter does not.

The ſame charge has been made, you know, by Dr. Prieſtley: and, truly, the paragraph quoted by both of you, gives ſome foundation for it; but nothing was farther from my intention than to make ſuch an aſſertion, or to convey ſuch an opinion, and I can readily explain it.

The Doctor had advanced againſt the opinion of the black oxyd of iron containing oxygen, that it did not oxygenate the muriatic acid: whereas, the red oxyd of lead, and other ſubſtances containing oxygen, produced that effect.

This objection I conſidered as frivolous, and did, as the Doctor has ſince ſaid, make very light of it, ſo that I did not pay much attention to the conſtruction of the ſentence deſigned for its anſwer. I intended it ſhould have been underſtood, it did not at all follow, becauſe muriatic acid could ſeparate a certain portion of oxygen from an oxyd of lead, it ſhould likewiſe ſeparate ſome from one of iron; and the more eſpecially, as the oxyd of lead, which will thus give up part of its oxygen, contains the greateſt quantity of it with which the metal can be united: whereas, that in the black oxyd of iron is ſmall in compariſon to the quantity with which the iron can be combined.

To be ſure, this compariſon was not neceſſary, but it was made as affording a corroborating proof of the frivolity of the Doctor's objection, and it ſeems to me to be a ſtrong

one: for the yellow oxyd of lead will not oxygenate the muriatic acid; and from the experiments of Macquart and Vauquelin, it appears, that iron may be joined to enough of oxygen to produce that effect.

The word "comparatively," in the ſentence you quote, was really intended to mark the compariſon between the different degrees of the oxygenation of the iron; and as ſoon as Dr. Prieſtley's pamphlet was ſent to me, I made a note on its margin to that effect.

After having made the charge I have juſt anſwered, you give an account of ſome experiments; and, having let me know my ſuppoſed opinion reſpecting the relative quantities of oxygen in the oxyds of lead and iron is void of foundation, you tell me "The true reaſon that red lead will oxygenate the "muriatic acid, and that a calx of iron will not, is that the "former readily gives its oxygene to the acid, and the latter "does not, owing to a difference in the elective attractions "ſubſiſting between the acid, oxygene, and the two metals."

The obſervation on the ſuppoſed opinion does not apply to me, as I never entertained it.

Your reaſon for the difference in the effects of the red oxyd of lead and black oxyd of iron on the muriatic acid is not ſufficient. It was not neceſſary to give one in my lectures on combuſtion; but ſince you have directed my attention to it, I will now offer what ſeems to me to be "the true reaſon."

Metallic ſubſtances muſt be united to a quantity of oxygen before they can combine with acids, but they may be joined to more than is neceſſary for that purpoſe. The quantity is different for each metal, and, perhaps, for the ſame metal with different acids; nay, for the ſame metal with different proportions of the ſame acid. Red oxyd of lead contains more than the requiſite quantity, while black oxyd of iron, in general, does not. When the red oxyd of lead is mixed with the muriatic acid, the lead, with the due proportion of oxygen, unites with ſome of the acid; while the ſuperfluous oxygen, being diſengaged, joins to and oxygenates another portion of the acid; and it is more than probable its ready ſeparation from the lead is promoted by the diſpoſition which it has to form this combination. But, when the black oxyd of iron is added to the ſame acid, there being nothing ſuperfluous in the mixture, the two ſubſtances unite calmly together.

You may eaſily ſatisfy yourſelf of the truth of what I have ſaid. Pour the muriatic acid on the red oxyd of lead: the oxygenated muriatic acid gas will be immediately diſengaged, and occaſion an effervescence. What remains will, in part, be converted into a muriate of lead; and the whole of it will be thus changed, provided the acid be in ſufficient quantity, and the ſurface of contact between it and the oxyd be frequently renewed by trituration in a mortar, or by boiling.

Make the red oxyd of lead, ſlowly, red-hot, and keep it, for ſome time, at that temperature—a part of its oxygen will be ſeparated, and may be eaſily collected. The reſt, in union with the lead, will form a ſubſtance, which, when cooled, will have a yellowiſh colour. Treat this yellow oxyd with the muriatic acid, in the manner directed for the red, and you will obtain the ſame muriates, but no oxygenated acid. And, laſtly, ſubject the black oxyd of iron to the ſame operations, and you will get only a muriate of iron.

You proceed, and take notice of my having remarked, Dr. Prieſtley was miſtaken in ſaying that finery cinder would not acquire ruſt; and you tell me, that, to determine the queſtion, you expoſed the ſcales which the blackſmiths ſtrike from red-hot iron to the air for twelve months, and that, notwithſtanding they were ſeveral hundred times ſprinkled with water, they remained free from ruſt.

Dr. Prieſtley alſo has accuſed me of "this too confident aſ-"ſertion;" and he has been pleaſed to ſay, it "would aſtoniſh "him if it was not too much of a piece with the reſt of my "performance."

I have often ſince made the experiment you mention, and generally with the ſame ſucceſs, not the ſmalleſt particle of ruſt being perceptible on the ſcales. However, you will not be ſurprized when I mention the circumſtances which led to the remark.

The Doctor had repeatedly declared finery cinder was the ſame ſort of thing with the ſcales of iron, and what was called, or, as he ſaid, dignified, by the name of the black oxyd. Mr. Fourcroy had aſſerted the black oxyd ruſted ſooner than iron. A ſubſtance which I did then, and do now, conſider as a black oxyd of iron, was ruſting before my eyes whilſt I wrote my lecture. And I had many times ſeen ruſt prepared by apothecaries, from a mixture of iron turnings, filings, and ſcales; and I have been often told, the ſcales increaſed the quantity, and haſtened the formation of the ruſt.

But you affirm, "The pure ſcales never will acquire ruſt; for, "when bar-iron is converted into finery cinder, it parts with "the ſmall quantity of coal it contained, and abſorbs oxygen "and water."

It has not been proved the ſcales of iron contain water. Their not ruſting ſeems to be owing to a very different circumſtance. But I will not trouble you with my experiments on this ſubject: they will be reſerved for Dr. Prieſtley.

You then acquaint me, you have repeated the boaſted of experiment with charcoal and iron ſcales, which, I have ſaid, as performed by Dr. Prieſtley, was of no value; and, having informed me you obtained the ſame reſult with him, you intimate, "it has puzzled every one to whom it has been men-"tioned;" but that, for your part, you "do not think it af-"fects the antiphlogiſtic ſyſtem; for the ſcales of iron con-"tain water, and retain it in ſo obſtinate a manner as not to "part with it upon the application of heat; but when coal is "added to the finery cinder, it takes away the water, by hav-"ing a greater affinity to it than to the calx of iron. The "coal decompoſes this water; its oxygen unites to part of the "coal, and forms the carbonic acid; while its hydrogen is ſe-"parated, diſſolves another part of the coal, and forms the car-"bonated hydrogen gas."

There can be no objection to your mode of performing the experiment. From your attending ſo ſtrictly to my obſervations on Dr. Prieſtley's, it would ſeem you did not think them void of foundation.

Who they are to whom you mentioned the experiment I do not know: they may be great lovers of wiſdom, but they have been little accuſtomed to conſider the ſubject in queſtion. Certainly the experiment does not affect the antiphlogiſtic ſyſtem; but, as I cannot admit, till I meet with proof, that the ſcales of iron contain water, I muſt reject your explanation.

I, too, have experimented on this ſubject, and have endeavoured to go a little farther than you. I have tried to aſcertain the nature of the remainder, as well as the quantity and kind of the volatile products. It was chiefly from his not having done ſo, that I ſaid Dr. Prieſtley's experiment was of no value; and as I am ſtill of opinion, unleſs it can be done, no concluſion for the determination of the ſubject in diſpute can be drawn from the experiment, I cannot entertain a more favourable opinion of yours.

My experiments will be repeated; and, if the reſults turn

out as they have hitherto done, it will appear the ſcales of iron, when mixed with perfect charcoal, and heated, loſe no more weight than what is equal to that of the oxygen, which the antiphlogiſtians believe they can prove to be neceſſary for the formation of the product obtained.

If I have not been miſtaken, then, the carbonic acid gas is owing to the union of ſome of the oxygen in the oxyd of iron, with carbon of the charcoal, and the carbonated hydrogen gas to the ſolution of carbon in hydrogen, both furniſhed by the charcoal.

It cannot, as has been already mentioned in my lectures, be objected to this explanation, that the charcoal affords an inflammable gas at a lower temperature when mixed with the ſcales than when uſed by itſelf; for, we know, when carbon and hydrogen are united in certain proportions, as, for inſtance, in the moſt perfect charcoal which has been made, the compound is ſolid at a very high temperature; but, when the proportion of hydrogen is greater, as in carbonated hydrogen gas, it is eaſily made gaſeous. Now, in the experiment under conſideration, the proportion of the hydrogen is increaſed by the union of part of the carbon with ſome of the oxygen in the oxyd of iron.

Neither can it be objected to this explanation, as you have very improperly done to Dr. Prieſtley's, that an oxyd of iron cannot be revived at the temperature to which the ſcales and charcoal are expoſed; for, although the iron does not become perfectly metallic, yet it approaches more nearly to that ſtate than it does before the operation, and thus affords an additional proof of the generality of that law of chemical combination, that a body attracts or retains different proportions of another with unequal force.

Having done with your particulars, you write rather a threatening paragraph: "Should you conſider," ſay you, "the objections of Dr. Prieſtley once more, and advance no-"thing but what is founded upon your own experiments, you "may hear from me again; and I promiſe not to be the firſt "to drop the ſubject."

It has been already intimated, in my letter to Dr. Mitchill, which you have ſeen, that it was my intention to reply to Dr. Prieſtley; and I cannot think I will be deterred from doing ſo, becauſe you have threatened to be my impugner: on the contrary, you are welcome to make whatſoever obſervations you may chooſe on my performance, and I can ſee no material

objection to their being communicated to the public under cover of an addreſs to me.

At the ſame time, be informed, you will write to one who is far from being a punctual correſpondent; even his friends complain their letters are unanſwered; ſo that, it is more than probable, he will take no notice of your criticiſms.

Do not underſtand, from this, I mean to deter you from writing to me. Your letter has afforded not a little entertainment; and, if you can always furniſh me with the like, it will be very acceptable.

Your maxim, that "mere aſſertions only ſerve to fix errors "deeply on the mind, and do not advance the cauſe of truth," will be, henceforth, I hope, ſo deeply impreſſed on your own mind, that your next eſſay will contain fewer than the one which is now anſwered.

In your concluding paragraph, you, doubtleſs, intended to be very ſevere; indeed, from the whole tenor of your epiſtle, it ſeems to have been written at a time when you were rather out of humour.

I am ready to attribute to you the beſt intentions, and to believe you have been induced to treat me in the manner you have done, from the notion of my having been wanting in reſpect to a man, to whoſe exertions Chemiſtry owes much of its advancement, and to whom, perhaps, you are under perſonal obligations.

Without doubt, there are ſeveral paſſages in my lectures, which, had I to write them over again, I would alter, not that my ſentiments, either with reſpect to his opinions or experiments, are in any degree changed, but the expreſſions, in ſome places, are liable to be miſinterpreted, and, in others, do not convey exactly my meaning. If, however, you will be at the trouble to read them over calmly and diſpaſſionately, juſt as they are, you will perceive there is no affront offered to Dr. Prieſtley. But, even if I have been petulant in my remarks, it is ſurely too ſevere to condemn me to a conſtant perſecution. Why, the Doctor himſelf does not think I deſerve it; for, after having treated me roughly, I had almoſt ſaid abuſed me, in his Conſiderations on Phlogiſton, part the ſecond, and been witty at my expenſe, in a letter to Dr. Mitchill, he has invited me to an amicable diſcuſſion of the ſubject, and expreſſed a regret at the publication of his letter.

For my part, I am perfectly diſpoſed to forgive whatever

he has ſaid of me; and ſurely, then, it will not be worth your while to loſe your temper about us: indeed, it would be ſilly in any one, but more particularly in you, to do ſo. A haſty, vindictive, and overbearing diſpoſition, is very unbecoming a philoſopher, and that whether he be a veteran or a novice in ſcience.

Let us, then, be friends; and if you are determined to controvert whatever I may ſay, take care that you do not ſubſtitute hard words in place of ſolid arguments.

I am your humble ſervant,

JOHN MACLEAN.

ARTICLE IX.

Caſe of the DELETERIOUS EFFECTS *of* OPIUM *remedied by the Excitement of Pain.*

By VALENTINE SEAMAN, *M. D.*

HAVING ſo frequently obſerved the great quantity of opium that a perſon, under the operation of acute pain, will take, without having any ſoporific effects induced by it, I have long been of the mind, that pain might be uſefully excited to remove the deadly influence of a large doſe that may have been previouſly taken. This idea I intimated in my Inaugural Diſſertation, publiſhed in 1792.

Yeſterday I had an opportunity of putting my principles to the teſt of experiment, being called to ſee the wife of —— Head, in Water-ſtreet, who had, about two hours before, taken an ounce of laudanum, and then lay in a deadly ſtupor, from which all the efforts of her friends were inſufficient to awaken her. Attempts had been made to get ſome vinegar into her ſtomach, but, I believe, with little effect; nor did I ſucceed much better in endeavouring to give her a doſe of white vitriol. I then procured a ſmall ſwitch, and applied it pretty freely to her arms and ſhoulders, which were defended only by a thin linen covering. I alſo applied ſome ſtrokes to her legs. In the courſe of a very ſhort time, indeed almoſt immediately upon the application of this remedy, ſhe rouſed

up, and begged me to deſiſt. She continued, for a time, much confuſed, with involuntary turns of laughter. Two ſcruples of white vitriol were then adminiſtered, followed, in about fifteen minutes, by half a dram of ipecacuana; notwithſtanding which, and alſo having her throat repeatedly tickled with an oiled feather, it was near an hour before ſhe could be made to puke: however, finally, ſhe puked, and, by the aſſiſtance of frequent draughts of warm water, her ſtomach was pretty thoroughly evacuated.

By the aſſiſtance of her friends ſhe was kept awake, or, at leaſt, ſlept but little at a time during the night, and this morning appears entirely recovered.

V. SEAMAN.

7th Month 2d, 1799.

ARTICLE X.

To the EDITORS *of the* MEDICAL REPOSITORY.

Philadelphia, July 10, 1799.

Should the following Obſervations, made on a ſmall Tour through the lower Parts of New-Jerſey, appear of ſufficient Importance, you will pleaſe to inſert them in your Repoſitory.—My Rout was from Philadelphia to the Mouth of Great Egg-Harbour River; from there, by the Way of Batſtow and Mount-Holly, home.

I am, &c.

THOMAS P. SMITH.

GEOLOGICAL REMARKS.

IMMEDIATELY on leaving the Pennſylvania ſhore you leave the granite ridge, and find nothing but a looſe ſandy ſoil, the greater part of which appears to be ſilex, interſperſed with ſmall rounded ſilicious ſtones. I found no ſtone, of any conſiderable ſize, but a brechia, compoſed of rounded ſilicious pebbles, imbedded in a very ferruginous *ce-*

ment. The foundations of the houſes in this country, which are all of wood, are formed of this brechia, except on the ſides of creeks, where the veſſels which trade to ports in the granite ridge bring it for ballaſt. In digging wells of a very conſiderable depth, nothing more has been found than the ſilicious ſand, ſmall rounded pebbles, and brechia.*

The whole country is ſo flat that I could not diſtinguiſh the ridge which ſeparates the waters that fall into the ocean from thoſe which fall into the Delaware. The coaſt, on the Atlantic, goes off very regularly into the ocean, at a ſmall angle, and is compoſed of exactly the ſame materials as the whole internal country—a looſe ſand and ſmall rounded pebbles. The pine barrens, which include the whole country, though interſperſed with other trees and underwood, are devoid of all vegetable mould: but this may be owing to the ſand being ſo looſe as to ſuffer it all to be waſhed into the earth.†

Query. From theſe circumſtances may we not conſider this country of very recent formation?

Bog Iron Ores.

I viſited none of theſe iron ores, which abound in this part of Jerſey, of a very excellent quality, but thoſe at Batſtow, which lie on the head waters of Little Egg-Harbour River, being informed by the gentleman who conducts thoſe works that they are all alike. This ore is formed in a bog, on the ſides of Little Egg-Harbour River, which is here a ſmall rapid ſtream. It is chiefly found where the water juſt oozes over the ground. In all probability it is the reſult of vegetable decompoſition, as the whole country round is nothing but ſand. Its formation is very rapid. The director of the works informed me, that he has known one bed, three or four inches thick, to be renewed three times in twenty-five years. As I ſuſpected the iron to be brought from the cedar

* One man, in reply to my queſtion, Whether they ever found any large ſtones in digging beſides the brechia, which I pointed out to him, replied, with ſome exultation, "O, yes, we ſometimes find one, in digging, as large as my head!" From his deſcription theſe *precious ſtones* muſt be rounded pieces of *amorphous quartz.*

† A view of this country would almoſt have ſuperſeded the neceſſity of Van Helmont's experiment of growing a willow in ſand, with diſtilled water. All the woods here grow under theſe circumſtances.

ſwamps above, I teſted the water that was almoſt ſtagnant over the ore, and, to my great ſurprize, found that it afforded no precipitate with the gallic acid, nor with pruſſiate of potaſh. When I added ſome acetic acid to the latter, a little blue precipitate was diſengaged, but not more than might be accounted for by the iron contained in the pruſſiate of potaſh. I had then the curioſity to teſt it with nitrate of quickſilver, acetate of lead, ſulphate of iron, and cuprum ammoniacum, and it afforded no precipitate; ſo that it appears as free from mixture as diſtilled water. On the ſurface, however, there was the uſual appearance of chalybeate water, but not to a very great extent.* There might, then, be a minute quantity of iron in the water, but too little to be detected by my teſts. On my return to Philadelphia I examined my teſts, and found them to be good. How, then, is this iron ore formed? Is the iron, on the vegetables being decompoſed, ſo far oxyded as to be inſoluble, and, therefore, only ſuſpended by the water, and depoſited as ſoon as it remains at reſt?

WATERS.

I teſted the well-waters in the barrens, and found none of them contained any muriatic acid. This is an extraordinary circumſtance, as I never teſted any waters in the granite ridge without finding it, though more diſtant from the ocean. This is ſo general, that as ſoon as the country in Jerſey, about nine miles to the eaſtward of Mount-Holly, began to wear a little the appearance of decompoſed granite, the water afforded a copious precipitate with nitrate of mercury.† Can there *poſſibly* exiſt any connection between theſe circumſtances?

I teſted a well at the mouth of Great Egg-Harbour River, though within a few hundred yards of the ſalt water; it gave ſcarcely a viſible precipitate. Does not this fact entirely diſprove a theory which has lately been attempted to be eſtabliſhed, of the capillary origin of ſprings and rivers? The water of this well was about on a level with the ocean. If, then, its waters paſs ſo far inland, and ſo high as to burſt out in ſprings on mountains, as is pretended, why was not this well impregnated with muriate of ſoda, as it is in a looſe ſoil,

* Wherever the water oozes through places where vegetables are decompoſing, there is this appearance: but ſuch waters ſeldom contain iron enough to be precipitated by the moſt delicate teſt, as I have often experienced.

† The nitrate of ſilver is, perhaps, a better teſt; but this is certainly accurate enough for all practical purpoſes.

with every advantage for that purpoſe? It cannot be pretended that the water is deprived of the ſalt by filtration, as it is well known that no operation of this kind will ſeparate ſalt when diſſolved.

ARTICLE XI.

On the Vapour which riſes from the Surface of the River St. Lawrence during the ſevere cold of Winter. In a Letter from Mr. F. BLANCHET, *of Quebec, to Mr.* MITCHILL, *dated Eſſex County (N. J.), July* 9, 1799.

SIR,

HAVING had opportunities of examining the effects of cold upon the human body, I have been induced, by a ſort of neceſſity, to inqure into its general effect in the phenomenon of *the freezing of water.*

It appears to me, that the philoſophers who have treated this ſubject, have not expreſſed themſelves in a manner ſufficiently exact, and do not ſeem to have comprehended the whole of the proceſs. The object which I have in view, and which does not appear to have been handled right, is the vapour which riſes from the ſurface of a river, &c. during the ſevere colds of the winter. This vapour does not proceed entirely from the caloric which is diſengaged from the water during its congelation; but, as I judge, is occaſioned, principally, by the ſuperabundance of caloric which is accumulated in that fluid; the reaſon whereof I take to be this: during the congelation of water, in general, the caloric, which preſerves it either in a gaſeous or liquid ſtate, is ſet looſe, *alone*, or in a *ſeparate* form, whenever the particles or globules of water are condenſed into ice. But the vapour which riſes from the ſurface of a river or a rapid that is in a ſtate of congelation, is occaſioned by the caloric which is accumulated under the ice.

For, I view it to be a well eſtabliſhed fact, that the internal heat of the earth is the cauſe of the fluidity of ocean-water, as well as of the water of great and ſmall rivers, &c. and

there is no doubt that, without this interior heat of the globe, which is diffuſed inceſſantly toward its ſurface, the water of the River St. Lawrence, and other great and deep waters, would be changed, during the exceſſive colds of winter, to a clear and ſolid maſs of ice. But, fortunately for living beings, the ſubterranean heat prevents the occurrence of this accident.

Now, ſince this is not the caſe, and ſince there is diſengaged from the central parts of the earth a certain quantity of caloric, the floating ice with which the river is covered being a bad conductor, and an impediment to its uniform eſcape, its accumulation below renders the water warm, and, conſequently, produces the exhalation, or moiſt-like meteor, which we obſerve between the maſſes of ice, or at the air-holes.

Some perſons have remarked, that water, at the freezing point, was colder than ice. If this is the fact, how can we conceive that water, which, according to this idea, contains a ſmaller quantity of heat than ice, can furniſh, on becoming congealed, to the ſurrounding water, a degree of heat ſufficient to elevate and turn it to vapour?

Thus, Sir, I have every reaſon to believe, that the vapour, or exhalation, obſervable above the ſurface of a river, while it remains frozen (except in certain ſpots, and thoſe the ſpots whence the vapour riſes), is owing, in a great meaſure, to the ſubterranean heat which, in ſuch circumſtances, is accumulated in the water beneath the bridge or cover of ice.

This miſtake, on the part of philoſophers, proceeds, as I apprehend, from their having made no diſtinction between the congelation which takes place upon a ſmall ſcale, or in a veſſel, and that which happens upon a great ſcale, as in a large river, &c. This, therefore, is a matter particularly to be attended to; ſince the central heat, which is continually extricated, ought to make a pointed difference in the circumſtances of the two caſes.

I am, with the moſt profound reſpect,
Sir, your very humble and obedient ſervant,

F. BLANCHET.

ARTICLE XII.

FACTS AND REMARKS ON THE ANTISEPTIC POWERS OF LIXIVIAL AND OLEAGINOUS SUBSTANCES: *Communicated by Mr.* F. BLANCHET, *to Dr.* MITCHILL.

Account of the Guanchios, *or ancient Inhabitants of the Iſland of Teneriffe, given by a judicious and inquiſitive Man, who lived twenty Years in it, as a Phyſician and Merchant.* (Extracted from the Hiſtory of the Royal Society of London).

"SEPTEMBER the third,* about twelve years ſince, he took his journey from Guimar (a town inhabited, for the moſt part, by ſuch as derive themſelves from the old *Guanchios)*, in the company of ſome of them, to view their caves, and the bodies buried in them. This was a favour they ſeldom or never permit to any (having in great veneration the bodies of their anceſtors, and, likewiſe, being extremely againſt any diſturbance of the dead); but he had done ſeveral *cleemoſynary* cures amongſt them (for they are generally very poor; yet the pooreſt thinks himſelf too good to marry with the beſt Spaniard), which endeared him to them exceedingly; otherwiſe it is death for any ſtranger to viſit theſe caves or bodies.

"Theſe bodies are ſewed up in goat ſkins, with thongs of the ſame, with very great care, particularly in the incomparable exactneſs and evenneſs of the ſeams; and the ſkins are made very tight, and cloſe to the body. Moſt of theſe bodies are entire: the eyes cloſed, hair on the head, ears, noſe, teeth, lips, beard, likewiſe the *pudenda* of both ſexes, all perfect, only diſcoloured, and a little ſhrivelled. He ſaw about three or four hundred in ſeveral caves. Some of them are ſtanding, others lie on beds of wood, ſo hardened by an art they had (which the *Spaniards* call *curar*, to cure a piece of wood), that no iron can pierce or hurt it. He ſays, that one day, be-

* The year is accidentally defaced.

ing a hunting, a ferret (which is much in uſe there), having a bell about his neck, ran after a coney into a hole, where they loſt the ſound of the bell. The owner, being afraid he ſhould loſe his ferret, peeping about the rock and ſhrubs, found the mouth of a cave, and, entering in, was ſo affrighted that he cried out. It was at the ſight of one of theſe bodies, very tall and large, lying with his head on a great ſtone, his feet ſupported with a little wall of ſtone, and the body reſting on a bed of wood (as before was mentioned). The fellow, being now a little out of his fright, entered it, and cut off a great piece of the ſkin that lay on the breaſt of this body, which, the Doctor ſays, was more flexible and pliant than ever he felt any kid's leather glove, and yet ſo far from being rotten, that the man uſed it for his flail many years after.

"Theſe bodies are very light, as if made up of ſtraw; and, in ſome broken limbs, he obſerved the nerves and tendons, and alſo ſome ſtrings of the veins and arteries, very diſtinctly.

"His great care was to inquire of theſe people what they had amongſt them of tradition, concerning the embalming and preſervation of theſe bodies. From ſome of the eldeſt of them (above a hundred and ten years of age) he received this account—that they had, of old, one particular tribe of men that had this art amongſt themſelves only, and kept it as a thing ſacred, and not to be communicated to the vulgar. Theſe mixed not with the reſt of the inhabitants, nor married out of their own tribe, and were alſo their prieſts and miniſters of religion: that, upon the conqueſt of the *Spaniards*, they were moſt of them deſtroyed, and the art loſt with them; only they held ſome traditions yet of a few ingredients that were made uſe of in this buſineſs. They took butter of goat's milk (ſome ſaid hog's greaſe was mingled with it), which they kept in ſkins for this purpoſe: in this they boiled certain herbs: firſt a ſort of wild lavender, which grows there, in great quantities, on the rocks. Secondly, an herb called lata, of a very gummy and glutinous conſiſtence, which now grows there on the tops of the mountains only. Thirdly, a kind of cyclamen, or ſow-bread. Fourthly, wild ſage, growing plentifully in this iſland. Theſe, with others, bruiſed and boiled in the butter, rendered it a perfect balſam. This prepared, they firſt embowelled the corpſe, and, in the poorer ſort, to ſave charges, they took out the brain behind;

theſe poor were alſo ſewed up in ſkins with the hair on; whereas the richer ſort were, as was ſaid before, put up in ſkins ſo finely and exactly dreſſed, that they remain, moſt rarely, pliant and yielding to this day. After the body was thus prepared, they had in readineſs a *lixivium*, made of the bark of pine trees, with which they waſh the body, drying it in the ſun in ſummer, and in ſtoves in winter; repeating this very often. Afterward they began their unction with the balſam, both without and within, drying it again as before. This they continued till the balſam had penetrated into the whole habit; and the muſcles, in all parts, appeared through the contracted ſkin, and the body became exceedingly light; then they ſewed them up in the goat-ſkins, as was mentioned already. He was told by theſe ancient people, that they have above twenty caves of their kings and great perſons, with their whole families, yet unknown to any but themſelves, and which they will never diſcover. Laſtly, he ſays, that bodies are found in the caves of the *Grand Canaria* in ſacks, and quite conſumed, not as thoſe in *Teneriffe*."

Remarks of Mr. B. on the above Account.

It ſeems to me that nature has explained herſelf in a more intelligible, and leſs equivocal manner to theſe ſavages than to our civilized people; or, rather, our prejudices of every kind render us deaf to her inſtructive voice: for, theſe children of Nature have accompliſhed, by inſtinct, what many centuries have not been able to teach our haughty philoſophy. It is only by a long ſeries of labours and experiments, that we have finally got back to this primitive ſimplicity.

From this barbarous people I turn my view to the inhabitants of ancient Egypt, who have been a ſubject of admiration in all ages. Theſe, whom every thing around them concurred to render both knowing and ſuperſtitious, have left us unchanging monuments of this ſtriking contraſt. Expoſed, by turns, to a crowd of bleſſings and of evils, this people was neceſſarily the ſubject of their influence. And, indeed, a ſoil which, one while, furniſhed them with grain in abundance, at another time refuſed its ſupplies, and was unproductive, and, then again, was ſurcharged with ſeptic poiſon, might be expected to diffuſe gaiety or ſcatter terror into their

hearts. To theſe principal cauſes we may add a number of ſecondary ones; ſuch as the rapid and numerous production of inſects, reptiles, and other devouring creatures, which infeſted, from time to time, the land of their reſidence. Such a contrariety of productions and conditions might be expected to ſharpen their faculties, rouſe their imaginations, and make them, at once, a knowing and ſuperſtitious race.

Be theſe things as they may, I ſhall, for the preſent, content myſelf with offering a few reflections on their manner of embalming their dead. The mummies are contained in ſubterranean vaults, and are, at this day, the admiration and aſtoniſhment of travellers. It has been laboured, with all aſſiduity, to diſcover this art, which has been loſt in the ages of ignorance and barbariſm. If, however, attention be paid to the characters and manners of this people, and, more particularly, to the climate in which they dwelt, it will be ſeen, that the *art of embalming their bodies* might be expected to have been begun, and to have grown to perfection, among them: for, mankind, in general, being more or leſs deſirous of perpetuating the memory of their fathers, this ſame deſire of immortality ought to have been powerfully felt by a people of warm and irritable minds, who would, conſequently, engage with ardour in ſeeking out the means.

Beſides, the exceſſive heat of their climate, and the peſtilential atmoſphere which they breathed, and of which they were ſo frequently the victims, would quicken, exceedingly, the progreſs of putrefaction in a body juſt dead. From this rapidity of decay aroſe the neceſſity of daubing theſe bodies with odoriferous ointments, to counteract the fetid exhalations which proceeded from them. This firſt ſtep led, with haſty ſtrides, to the diſcovery of the art they were ſeeking, ſince *greaſy ſubſtances reſiſt all manner of putrefaction*, and are properly ranked among the agents which retard or counteract that proceſs.

Therefore, by the rule of analogy, it may be inferred, with the higheſt probability, that the deſire of guarding the living againſt an infected and impure air, led the Egyptians, accidentally, to the mode of embalming the bodies of their dead, or made them early perceive the efficacy of the means of doing it; while they were endeavouring to repreſs the exhalations from putrefying carcaſes. From this rude beginning the art would be improved and refined, in proportion to the progreſs of wealth and luxury.

The proceſs by which the old natives of Teneriffe embalmed or preſerved their dead, is certainly a proof of the generation of certain acids in a corrupting corpſe. The *fat* which generally is abſorbed, or diſappears before death (provided the death is not violent or ſudden), is a further proof that the prevailing acid is not the carbonic. Then, if the *lean* and *muſcular* parts are chiefly thoſe which are left, we may conclude that the oxygene, aſſiſted by caloric, will connect itſelf with the ſepton (azote) of theſe ſubſtances, and engender or conſtitute the ACID OF PUTREFACTION.*

The proceedings of this ſavage people to prevent the total diſorganization of their dead, are founded on the nature and reality of things; that is to ſay, by penetrating the carcaſes with *lixivium* or *ley*, they neutralize thereby every acid that can be formed; and thus oppoſe the work of corruption. Such, O poliſhed nations! are the ſagacity and knowledge of the people whom we deſpiſe. It ſeems as if nature had been pleaſed to inſtruct theſe humble beings in the ſecrets of her works, while ſhe has treated *our* pride with diſdain.

F. BLANCHET.

Eſſex County (New-Jerſey), July 6, 1799.

* See Mitchill's letters on the doctrine of ſepton, of which there is a catalogue in Med. Repoſ. vol. ii. p. 97—98; and, for the pieces written ſince that enumeration was made, ſee the ſame volume, p. 292—vol. iii. p. 14—New-York Monthly Magazine for July, 1799, p. 264—the London Monthly Magazine for March and April, 1799—Hardie's Account of the Malignant Fever at New-York, in the year 1798—and an abſtract of much of the doctrine, in the appendix to 2 Trotter's Medicina Nautica, printed in London, 1799—ſome account of its application to explain the phenomena of fever, &c. in Tilloch's Philoſophical Magazine for June and July, 1799.

ARTICLE XIII.

Speculations concerning the perſpirable Fluids of Human Bodies, with the View of aſcertaining how they are ſometimes converted to ſeptic or peſtilential Matter. In a Letter from Dr. MITCHILL *to* ANDREW DUNCAN, *M. D. &c. Profeſſor of the Inſtitutes of Medicine in the Univerſity of Edinburgh, dated Auguſt* 11, 1798.

SIR,

IN reply to your laſt letter from Edinburgh, I have to remark, that, of the fluids afforded by the human body, few have been more talked of, though, perhaps, none to leſs advantage, than thoſe exhalations or effuſions from its ſurface, denominated *perſpiration* or *ſweat*. The reaſon alleged wherefore ſo little has been experimentally done upon theſe fluids or humours, is, that they cannot be obtained in ſufficient quantity; their volatility being ſuch as to prevent the collection of either of them, to ſuch an amount, that much certain information can be obtained about their peculiar natures and qualities.

This is, indeed, in ſome degree, the caſe; and, without doubt, the difficulty of procuring, conveniently and frequently, parcels of the matter ſecreted from the ſkin, has retarded the progreſs of inquiry on this point. While, therefore, the fluids ſecreted in the liver, pancreas, kidneys, ſtomach, mouth, and almoſt every other part of the human frame, have been examined with great care and exactneſs, experimenters are yet very much behind-hand in their knowledge of the cuticular diſcharge.

It is not my intention to ſtrike the balance between the proportions of fluids ſecreted by the kidneys and the ſkin, on the one part, nor to adjuſt the difference between the exhalation by arteries or glands, and the inhalation by lymphatics or abſorbents, on the other. My object is, to examine, by ſuch means as, it appears to me, we are poſſeſſed of, what ſubſtances they are which compoſe the cuticular ſecretion in the time of health, what alterations it undergoes by accumulation and confinement in certain caſes, and how peſtilential fluids and fomites may be produced thereby.

It will not, therefore, be neceſſary to give an anatomical

deſcription of the ſkin. All that will be offered will be capable of being comprehended without going into details upon that ſubject. Almoſt all that you need recollect is, that the *true ſkin* is furniſhed with arteries, veins, glands, excretory ducts, and lymphatics, and that it is covered over by a porous, ſemi-tranſparent ſort of ſheath, called EPIDERMIS, or CUTICLE, which is *inſenſible*, *bibulous*, and *exceedingly apt to grow naſty*; and this to a conſiderable degree without the conſciouſneſs of the wearer.

A quantity of oil, ſecreted into the cells of the adipoſe membrane, underlays the true ſkin in moſt parts of the body; and the veſſels and glands concerned in this function combine alſo a portion of the phlogiſton (hydrogene) and carbone of the blood, into a ſebaceous or greaſy compoſition, to anoint and ſoften the cuticle. A portion of this fatty matter, in the time of health, is daubed over the whole ſurface, though in greater quantity in ſome parts, and at ſome times, than at others. (Liquor oleoſus—inflammabilis—cutem perungit. *Haller*. 5 Element. Phyſiolog. § 20). The palms of the hands, the inſides of the fingers, and the ſoles of the feet, are remarked for having the leaſt of it.

This oily fluid, with which the external ſurface of the body is beſmeared, was found, in the early ſtages of ſociety, while yet dreſs was ſimple, and few or no clothes were worn, to be too copious in ſome conſtitutions, and to collect in greater quantity than the purpoſes of comfort required. When this happened, from its adheſive nature, particles of duſt, and other things floating in the air, would readily ſtick to it, and occaſion ſome degree of unpleaſant ſenſation. To remove this unpleaſantneſs, and the cauſe of it, inſtinct or accident led to the practice of *bathing*, or of purification with *water*; which being not capable of removing completely the foul and greaſy collection, was afterwards quickened in its activity by *alkalies* and *ſoaps*, ſubſtances which experience proved to be very efficacious in ridding the ſkin of its incumbrance. It ſometimes happened, too, that the oily liniment of the ſkin was too ſparingly effuſed, or too quickly removed, and did not impart to the cuticle (which, it will be remembered, has neither nerves nor ſenſibility) that pliancy and ſuppleneſs which is eſſential to pleaſant and perfect feeling. The epidermis was then prone to become horny, to crack, and to induce diſagreeable or painful ſenſation. To relieve this inconvenience, the cuſtom of applying *greaſe* or *oil* over the

ſurface of the body ſeems to have been adopted by mankind, likewiſe at a very early period; and, in this way, the deficiency of the natural ſecretion was ſupplied by art.

The uſe of water, and of alkaline lotions, ſometimes rendered more active by being warmed, and the employment of oil and ſoft ointments, now and then aided by *manual friction*, ſeem to have obtained very extenſively, from the mere comfortable effects they wrought, without any reaſoning or theory whatever. Sometimes a perſon employed the *watery* application only, ſometimes the *oily*, and then again he made uſe of *both*, in rapid ſucceſſion.

But this adipoſe ſecretion is by no means a *pure oil*. According to circumſtances, it is occaſionally blended with *phoſphoric*, *ſulphuric*, *ſeptic*, and a ſurpluſage of *carbonic* matter.

And it is further altered, as far as its nature will permit, by the conſtant tranſmiſſion of that AQUEOUS FLUID, the INSENSIBLE PERSPIRATION, through the cuticle, and by the drops of SWEAT into which it is ſometimes condenſed; both formed by a coalition of a portion of the phlogiſton (hydrogene) of the blood, with a parcel of its oxygene, and this liquid ſerving as a menſtruum for certain other ſubſtances. The exceedingly great quantity of perſpired matter inceſſantly paſſing off from every exhalant pore of the ſkin, leaves behind, as it evaporates, ſome of its more fixed, ſaline, and feculent parts, along with, or upon, the greaſy covering of the cuticle. A portion of carbone, phoſphorus, and ſepton, with, ſometimes, an overplus of oxygene, ſeem to accompany this effuſion of the ſkin, and, in ſome quantity, to remain behind, after the water has evaporated.

It was remarked before, that the cuticle was inſenſible, bibulous, and, of courſe, exceedingly apt to grow naſty. How can it be otherwiſe? The cuticle has neither nerves nor blood-veſſels, and there is little or no reaſon to believe that it poſſeſſes organization. It is, therefore, in ſtrictneſs, leſs a part of the living body, than a kind of tight-ſhirt, or cloſe-ſetting tunic, drawn over the whole ſurface of the body This ſhirt or tunic of cuticle may be conſidered as a foreign wrapper, or piece of natural clothing; and, like every other foreign thing which enwraps the body, is liable to become charged with ſuch ſubſtances as are excreted from the TRUE SKIN within it. The foulneſs of this NATURAL SHIRT is a frequent occurrence. Almoſt every body lets it get naſty.

When the naſtineſs is very conſiderable, the poiſonous or ſtimulant matter entangled there, irritates or inflames the organ of feeling, with which it lies in contact, and produces various itchings, pimples, eruptions, blotches, tetters, ſores, &c. being a PRINCIPAL CAUSE of the diſtempers of the ſkin. To a correct mind, *a dirty cuticle will appear to be a dirty ſhirt.* If the former could be ſtripped off, and cleanſed in the waſh-tub, like the latter, there might poſſibly be a convenience in it. This is, however, of ſmall moment, ſince we know the cuticle can be well cleanſed without being taken off.

The modern uſe of white linen and cotton, as articles of raiment, enables us to judge, with tolerable accuracy, what material it is, which ſhirts, made of thoſe kinds of cloth, and worn next the cuticle, receive from the PERSPIRATION and SWEAT. What the garments imbibe muſt have paſſed firſt through the exhalant arteries or excretory ducts, then undergone a further change while remaining in and about the CUTICLE or FIRST SHIRT, and afterwards have been tranſmitted, by rubbing or wiping, to the linen fibres or cotton filaments OF THE SECOND. The animal matter which befouls the OUTER SHIRT is a good indication of that which adheres TO THE INNER. For, as clean linen and cotton become ſoiled very ſoon by lying in contact with any part of the human body, what they receive muſt be principally derived from the cuticle which they touch and abſterge.

The uncleanneſs of SHIRTS, DRAWERS, and STOCKINGS, may, therefore, be deemed to be matter wiped from the CUTICLE, and derived to the cuticle from the veſſels and ducts of the ſubjacent TRUE SKIN. An examination, then, of the facts relative to clothing grown foul by long wearing next the cuticle, may be conſidered as virtually an examination of the cuticle itſelf, and of the excreted matters which ſtick to it.

I. One general property of all theſe matters, and of clothes tainted by them, is, that alkaline ſalts and ſoaps will ſeize, neutralize, and extract them; the garments being thereby rendered wholeſome and clean. An unqueſtionable effect of ſuch applications is to remove the *greaſy* or *unctuous* matter communicated to them from the cuticle. But this is not all—The *watery* as well as the *oily* part of the perſpirable matter contains the BASES OF ACIDS. A portion of oxygene combining with this fat material, imparts to it more or leſs of

rancidity. Another portion, uniting to carbone, forms carbonic acid, and this, volatilized by heat, is converted to carbonic acid gas. Another portion of the acidifying principle, uniting to the phoſphorus, turns it to phoſphoric acid: while the ſepton (azote), if it happens to combine with caloric alone, flies off in the form of azotic air; or, if it makes a junction with oxygene, conſtitutes, with it, SEPTIC ACID. Oils, being capable of uniting with the ſtronger acids, eſpecially with the ſeptic, and probably, alſo, with the phoſphoric and carbonic, there can be little doubt that theſe acids, or ſome of them, or ſome part of ſome of them, join with the adipoſe matter into a ſort of ACID SOAP, on the cuticle and ſhirt. This acidifying proceſs, whereby carbone, phoſphorus, and ſepton, unite to oxygene, takes place, NOT BEFORE SECRETION, but AFTER the perſpired matter is depoſited on the cuticle, or lodged in the ſhirt. The formation of acids, in this caſe, is not the immediate effect of vital movement at all, but reſults from chemical attraction going on among the particles of the fluids, AFTER EXCRETION, as in other inanimate matter; neither the cuticle, nor the fluids which beſmear and bedew it, having any living energy to controul or modify the laws of chemical action. Hence proceeds that SOUR SMELL from clothes in which ſailors and labouring men have ſweated profuſely, which, though obvious to the ſenſes, and familiar to the wearers and to waſher-women, remains unknown to hardly any body but philoſophers.

Thus a ſolution of an alkaline ſalt, or of a ſoap, detaches all *acids* from the cuticle and from clothes, as likewiſe from all oily or other connections they may have formed. And the ſame lixivial and ſaponaceous mixtures remove all *greaſy* ſubſtances, by virtue of the ſtronger attraction they exert toward theſe, than theſe poſſeſs for the cuticle, or the garment worn next to it. On the cleanſing of clothes there is one fact worthy of being mentioned. Sailors, on Weſt-India voyages, when deſtitute of ſoap, frequently waſh their clothes in water, WITH THE AID OF MOLASSES, as a ſubſtitute for ſoap. They find this practice anſwer pretty well. Sugar is known to decompound nitrous acid very readily. Is not the purification of mariners' clothes, in this manner, a circumſtance of weight, to evince the decompoſition of the ſeptic acid they contain, by the ſaccharine ingredient of the molaſſes?

Such is the detail of the principal conſtituent parts of that

naſtineſs which accumulates on the ſkins and in the garments of human beings; and ſuch the theory of the operation of alkalies, &c. in making them clean. From a compariſon of the facts relative to *perſpiration* with thoſe which reſpect *urine*, it will readily appear, that the two fluids very much reſemble each other, as might naturally be ſuppoſed from the great connection there has been long known to exiſt between the two diſcharges. What is, therefore, true of the one, is, to moſt purpoſes, applicable to the other. In particular, the ſweat as well as the urine contains *ſepton*, which often combines with oxygene, and forms that noxious compound, *ſeptic acid*.

Yet it muſt be remembered, that as extractive matter and acids are capable of ſolution in mere water, and require not the aid of alkalies to detach them, that waſhing the cuticle in cool, or, more readily, in warm water, removes every thing but the greaſe, and even looſens or liqueſies a part of that. Water cleanſes the cuticle or a ſhirt by exerting a greater attraction for the foul material ſticking to them, than the ſkin or a garment does. For this purpoſe the water muſt be clean, or, at leaſt, leſs tainted than the ſubject intended to be purified. Upon the ſame principle that *water* or *alkaline ley* cleanſes the cuticle, a *clean ſhirt* will do the ſame; that is, by attracting or abſorbing from it ſomething foul or foreign. *Clean ſhirts*, *drawers*, and *ſtockings*, are, therefore, ſubſtitutes for *clean water*: and he who changes his linen very frequently, deterges the ſurface of the body *almoſt* as much as by bathing; and, perhaps, for the purpoſes of health, *quite* as much. This appears to be more certainly the caſe, becauſe, after waſhing in ſoap-ſuds, a portion of alkali adheres to the cloth, unleſs it be well rinſed, and is inſtantly ready to exert its neutralizing effect. A dry ſhirt, thus waſhed, may be conceived as beſprinkled with innumerable minute cryſtals of potaſh and ſoda.

Pathologiſts have obſerved that *acid ſweats* have ſometimes occurred. In theſe caſes an aceſcent diſpoſition, or acid acrimony preſent in the blood, at the critical period of fevers, and at other times, may tincture the fluids ſecreted in the veſſels of the ſkin with ſome of their peculiar qualities. This has been alleged to have taken place to ſo high a degree as to turn *blue* litmus-paper to a *red*. It is not material to my argument whether this happens frequently or never. It is enough for my purpoſe that the liquids excreted on the cuticle have a

tendency to become acid, *after* being depoſited there. If *acid ſecretions* ever happen, they exhibit a ſtronger caſe, and illuſtrate the doctrine with additional force.

II. Another general property of theſe vitiated or corrupted animal excretions is to rot the clothing to which they adhere, and cauſe it to wear out the faſter. A ſhirt, indeed, worn until its threads are fully impregnated with foulneſs, requires ſo much ſoaking and friction, while in the waſh, that there can be no wonder if, in the operations of being made clean, it is almoſt wrung or rubbed to pieces. But, independent of all this, it is well known that linen, and clothing of all kinds, become very remarkably weaker by lying and remaining long in their dirt, without being ever handled by the laundreſs. There may be ſuch a collection and concentration of animal matter, as not only to rot the cloth, but to convert it to an infectious fomes, and make it diffuſe peſtilential vapours around. Garments, too long worn, without being cleanſed from time to time, often grow *rotten* and *infectious* as they grow *naſty*. Alkalies remove the naſtineſs, and, if applied diſcreetly, in ſeaſon, prevent the rottenneſs, and check the infection. Every day's experience demonſtrates their efficacy in theſe proceſſes. The following fact may ſerve as an exemplification of a very common occurrence: A gentleman, in the time of war, was taken captive, and compelled, by the enemy, to march on foot a great diſtance, at a violent rate, during very hot weather. He ſweated moſt profuſely. At the end of the march he was confined in priſon, and treated with great rigour. Being deprived of changes of raiment, and of the means of keeping himſelf clean, his ſhirt, in a ſhort time after his confinement, became rotten, and he fell ſick of a fever. In this inſtance, no doubt, both the cuticle and ſhirt became impregnated with *ſeptic poiſon*, manufactured from the *ſweat*, from which both the *conſtitution* and the *clothing* were the ſufferers. In this way the American and Britiſh ſailors, who viſit the Weſt-Indies, *catch yellow fevers*, as they are called, without once going on ſhore, or having any communication with infected perſons. Numberleſs facts prove that, even in the harbours, theſe fatal diſtempers are not *introduced from on ſhore, but bred and ſupported on board the veſſels themſelves*, in which the people fall ſick, whence they are carried on ſhore to be cured.

III. A third general effect of theſe ſeptic productions, as was obſerved in the preceding paragraph, is *to injure the con-*

ſtitution. This they do in two ways: 1. By a direct ſtimulant operation upon the part of the *true ſkin* to which they are applied, cauſing different forms of tinea, herpes, pſora, elephantiaſis, petechial and miliary eruptions, &c. &c. and, 2. By a direct ſtimulant operation upon the heart and ſanguiferous veſſels, after they are conveyed thither through the abſorbents ariſing from the cuticular ſurface, cauſing various grades of febrile diſturbance, of the intermittent, remittent, and continued forms, and inducing diſtempered motions in the lymphatic ſyſtem, as they paſs through its branches. All examiners of this ſubject ſhould, however, recollect, that ſeptic fluids, though they exiſt on the cuticle and in the blood-veſſels, do not always ſtir up diſeaſes; for the conſtitution may be, and frequently is, in ſuch a ſtate as to reſiſt their tendency to produce morbid action; the reaſon of which ſeems to be, that the ſtimulus no longer irritates a conſtitution which is ſeaſoned or habituated to it.

IV. A fourth general property of theſe fluids, engendered in corruption, is to be volatilized by the heat of ſummer and autumn in the United States. The noxious quality of the vapours proceeding from putrefying collections of common animal matter is too well known to require mentioning in this place. When that other animal matter which conſtitutes perſpiration and ſweat, and accumulates in the cuticle and clothing to large amount, has undergone the changes to which it is prone, it is much diſpoſed to aſſume the aërial form. Some portions of theſe exhaling vapours are often pungent, active, and of a deleterious quality. They are produced, generally ſpeaking, not in the circulating blood, nor during the proceſs of ſecretion, but *after their* depoſition upon the cuticle and clothes, and aided by HEAT and MOISTURE, through a proceſs ſimilar to that which converts urine, and ſeveral other excrementitious ſubſtances, into offenſive and noxious airs.

Clothing, or merchandize of any kind, wherein noxious airs or fluids of the ſeptic kind are generated or accumulated, is called a FOMES. In this way bills of paper-money, paſſing frequently from man to man, and becoming tainted with every thing that human hands can impart to them, have, in the city of New-York, degenerated to a *fomes.* So the ſhoes and ſtockings of that unclean perſon who ſuffers ſeptic acid ſo to accumulate about the toes and feet as to eat away the cuticle to the quick, may become a *fomes.* In like man-

ner a ſeaman's cheſt of dirty clothes, tainted with all kinds of animal exudations, may, during a voyage from Cape-François to New-York, turn to a *fomes*, though not a particle of infection was borrowed from the atmoſphere of St. Domingo, or from any perſon living or dying in it. By a proceſs in no wiſe different, do feather beds, and beds of all kinds, that have been long laid upon, and thereby become thoroughly impregnated with perſpirable and other matter, acquire their unwholeſome qualities, and turn to a *fomes*. Juſt thus are clothes charged with perſpirable and other excrementitious matter, diſcharged from the bodies of priſoners in jail, and of other perſons living in foul and confined places, converted to venom and peſtilence, or to an infectious or contagious *fomes*. And it is not impoſſible that a bale of cotton, or any ſimilar material, may, by impregnation with animal matter, become a repoſitory of miſchief, and *a fomes*.

In the preceding paragraph I have introduced the word "fomes" as often as I conveniently could; and have done ſo that I might ſhew the impropriety of the term, and offer my objections to it. The *literal* meaning of *fomes*, in Latin, refers to that quality, in bodies, which makes them catch and retain fire very readily; ſuch as punk, tinder, lint-ſtock, and touch-wood (rapuitque in *fomite* flammam—Virg. Æneid i. v. 180; in the note to which, in the Dauphin's edition, may be ſeen a blunder of the critics.) ACHATES uſed a *fomes* when he ſtruck fire with flint, &c. *Metaphorically*, it denotes an incentive or motive to any action. While the belief prevailed, that infection was carried from place to place, like a fire-brand, and paſſed from perſon to perſon, like a ſpark, the word "fomes" was borrowed to expreſs, by a ſtrong and illuſtrative figure, the analogy between the ſuppoſed effects of fire and thoſe of peſtilence. As each of the ſubſtances juſt named was a ſomes of fire, ſo a cheſt of clothes, a blanket, a bed, or a bale of merchandize, was a fomes of peſtilence. This mode of expreſſion was thought to be highly philoſophical and correct; inſomuch, that when any one affirmed that an atom of contagious matter, received into a blanket, multiplied itſelf as a ſpark of fire enlarges in a tinder-box, he was thought to expreſs himſelf ſenſibly.

But it will appear, to any one who comprehends the preceding obſervations, that the ſimile, if it be intended for a ſimile, is imperfect, or the metaphor, if it is meant for a metaphor, is deceitful: for it has been ſhewn, that the thing

called a *fomes* produces peſtilential matter without having a particle of infection (like a ſpark of fire, or even a bit of leaven) introduced into it from without; and it follows, of courſe, that all figurative expreſſions, calculated to uphold the idea *of foreign introduction* of common peſtilential matter, and its *derivation ab extrà*, are highly conducive to the continuance and propagation of error. It is high time this form of ſpeech was diſcontinued. This figurative expreſſion itſelf, and the unhappy aſſociation of ideas which ſprings from it, are the cauſe of a large proportion of the wrong-headedneſs now prevalent, concerning the diſtempers called infectious.

Was not the malignant fever, or plague, which deſtroyed a great proportion of one thouſand nine hundred and forty-ſix convicts, on board the hulks in the Thames (Colquhoun's Police of the Metropolis, chap. xii. p. 307), generated in the veſſels? and were there not local cauſes enough to excite it, without the importation and introduction of a fomes? Did not the peſtilential diſtemper which afflicted the Spaniſh captives at Wincheſter ariſe within the walls of their priſon, independent of any fomes introduced from without? Was not Hoſier's ſquadron, during the unfortunate expedition againſt Porto-Bello, diſmally diſabled by a diſtemper, bred on board and within the ſhips, wholly unaided by any thing like a fomes? And, if it was not ſuperfluous, I would aſk, whether malignant diſtempers did not harraſs, with their viſitations, almoſt all the great cities of Europe, and, indeed, of the world, until, by enforcing ſufficient laws againſt nuiſances, their inhabitants removed the cauſes?

In all theſe inſtances, dreadful as ſome of them were, the cauſes of the diſtempers were engendered or manufactured on the ſpot. Theſe exciting cauſes were, in all, exactly ſimilar to that which befouls a ſhirt, or the commoneſt article of clothing. If there is any difference of moment, it is nearly this: in one caſe peſtilence is manufactured upon a *great* ſcale, and, in the other, upon a *ſmall* one. The ſame principle pervades both caſes. The laws of nature operate uniformly in both: *and the ſame means that will cleanſe a ſhirt will purify a ſhip or a jail.* Subſtances that ſubdue acids, ſuch as OILS, CALCAREOUS EARTH, AND SOLUTIONS OF ALKALINE SALTS, CAN ACCOMPLISH THE WORK.

And if I interpret right that accurate mineralogiſt FERBER (Travels through Italy, letter xi. p. 129, edit. by Raſpe), the

catacombs, near the cities of Naples and Rome, are cut through and into maſſes of *tufo-ſtone*. This ſeems to conſiſt chiefly of volcanic aſhes, hardened into a ſubſtance of ſufficient compactneſs, in many caſes, for building: and thoſe afforded by Veſuvius, whether grey, white, brown, or yellowiſh, contain *alkaline* or *calcareous* particles enough to effervesce with acids. Hence the preſervation of human bodies in thoſe ſubterranean chambers can be eaſily accounted for. The lime preſerved theſe as the ſoda preſerved the mummies. SPALLANZANI has accounted for the preſence of this alkaline matter. In his very intereſting travels (Viaggi alle due Sicilie, &c. Pavia, 1792), he affirms, that a large tract of the Appenines, and the ſubſtratum of the volcanic country about Naples, conſiſt of the *carbonate of lime* (tom. i. p. 109—114). And as he has ſhewn that ſundry volcanic productions, particularly the vitrified lavas of the Lipari Iſlands, contain *muriatic acid* (tom. iii. p. 351), which, he thinks, is derived from the decompoſition of *common ſalt* (p. 361), *veramente del mare iſteſſo penetrante per di ſotto i monti volcanici*, penetrating from the ſea itſelf, beneath volcanic mountains, does it not follow, that its *ſoda* muſt be combined with other foſſils, and, perhaps, with the tufo?

There has been a number of tolerable conjectures on the preciſe nature of peſtilential matter. Beſides thoſe I have mentioned in other eſſays, I refer you now to the *Loimologia* of HODGES, a phyſician who reſided in the city of London during all the time of the plague of 1665. Obſerve how well he has gueſſed. In his § 1, on the Cauſe of a Peſtilence and a Contagion, he conſiders it as "ariſing chiefly from a "change or corruption of the *nitrous ſpirit* in the air" (p. 32 and 37). The *nitrous ſpirit* of this writer, as well as of his countryman and predeceſſor MAYOW, was evidently *vital* or *oxygenous air*; and, by its change or corruption, may be obviouſly underſtood, a combination of it with ſepton, the principle of putrefaction, or azote, the element hoſtile to life. To this change in the nitrous principle, Hodges aſcribes "blaſts upon trees and diſeaſes among cattle," as well as a peſtilence among mankind (p. 41); and to ſomewhat of *an acid*, ſometimes produced in this way, he attributes the quality of rain-water to ſtain the clothing and ſkins of travellers in the equatorial regions of Africa, and "burn upon them, "as it were, peſtilential characters" (p. 42). He affirms, too, what I have often witneſſed, that there is a great ſimili-

tude between a peſtilence and a ſcorbutic habit (p. 46); and declares (p. 49), that the convulſions and vellications of the ſtomach and alimentary canal are not owing to a putrefaction of bile, nor an eruption of fire, as the manner of thinking then was, but to this "vitiated ſaline ſpirit." This author makes favourable mention (p. 171) of ſpirit of hartſhorn (ammoniac), given in doſes of two ſcruples and one dram.

I ſhall, however, reſt the argument here for the preſent. If the reaſoning advanced eſtabliſhes the principle, then the principle ſo eſtabliſhed may be employed to explain innumerable other facts in nature belonging to this head. Your ſagacity will judge both of the rule and its application.

Be aſſured I am yours,

With high conſideration and reſpect,

SAMUEL L. MITCHILL.

REVIEW.

ART. I. *De Culturâ Radicis Brazilicæ; de Curâ Boum in Braziliâ; et de Culturâ Herbæ Nicotianæ in Braziliâ, Poemata Georgica: quibus adjicitur* PRUDENTII AMARALII *Brazilienſis de Sacchari Opificiò Carmen, &c.* 8vo. pp. 113. *Ulyſſipon.*

A VOLUME of American Georgics, written in Latin hexameter verſe, may be fairly pronounced a literary rarity: and yet the Portugueſe colony of Brazil has afforded ſuch a work. With great pleaſure we make known to our readers the literary labours of their countrymen on the ſouthern diviſion of the extenſive weſtern continent, and give them a curſory account of theſe pieces of didactic poetry, which, though not equal to the more laboured and formal PRÆDIUM RUSTICUM of the Jeſuit VANIER, are, however, performances of conſiderable merit and reſpectability. The author, though born in Oporto, appears to have been carried to Brazil very early, and to have been educated there (p. 61).

The firſt poem is on the cultivation of manioc (jatropha manihot), the plant whoſe root furniſhes the material of which caſſava-bread is made. It is divided into two books—the firſt of which treats of the culture, and the ſecond of the economical uſes of the vegetable. It is remarkable that the invocation, at the opening of the firſt book, is to St. Thomas the apoſtle, who, as the Brazilians report, penetrated into that region, and made the diſcovery of this moſt uſeful root; and, to prove it, they pretend to ſhow the impreſſions of his feet, as he walked over the rocks, ſtill remaining in many places. The author, whoſe name we regret that we have not been able to learn, after having given a circumſtantial deſcription of the manioc-plant, and particularly of its root, as well as of the manner of clearing the land of the natural growth of wood, and climbing vegetables, and alſo of the method of planting it, and the ſoil which ſuits it beſt, proceeds to enumerate the different animals who prey upon it, and the means of preventing their ravages: among others, he

is very minute on the ſubject of *ants*, as exceedingly injurious to the crop. We ſelect his deſcription of the mode of deſtroying theſe inſects by ſcalding water, as a ſpecimen of the ſtyle and talent of the writer.

——————— "Nimirum clauſtra ligone
"Convellunt, caecoſque cavos, et funditus omnes
"Perrumpunt cameras. Intùs ſecreta domorum
"Apparent reſerata: hic fractis horrea tectis,
"Plena latrociniis: illic cunabula prolis,
"Cuncta patent: trepidæ huc illuc, ſurſum atque deorſum
"Concurſant matres: fervet regnum omne tumultu.
"Tum verò expediunt flammis undantia ahena,
"Et ſuper invertunt: it largo flumine torrens
"Fumidus, et populum fugitantem ac multa timentem
"Obruit illabens, et funere mergit acerbo."

In the ſecond book, which treats of the ſcraping, grinding, ſoaking, and preſſing of the root, mention is made of the poiſonous quality of the ſweet liquor which is ſqueezed out from the plant while under preparation, and which kills brute animals as well as man. The white ſediment of this fluid is called the FLOUR OF MANIOC, and is an excellent part of the manufacture. If this liquor is boiled away to ſome thickneſs, it loſes its venomous quality, and acquires an agreeable taſte. The *cream-like* ſediment obtained by this operation is the TAPIOCA of commerce, and of medicine. The manner of procuring it is thus deſcribed:

"In fundo quidquid, defuſo humore, reſedit,
"Ad ſolem ſudumque loco curetur aprico,
"Perque dies aliquot, vivo de flumine ſumptis,
"Purgetur limphis, iterumque iterumque lavetur,
"Donec candorem adſciſcat, ſithonia qualem
"Nix habet: inde domi bene multis utile rebus
"Servandum: hanc etenim partem radicis et ipſe
"Commendat color excellens, et plurima virtus.
"In primis potis eſt triſtes depellere morbos.
"Seu fluxu infeſto pectus gravis afficit humor,
"Seu febris aſſiduæ paulatim eſt flamma medullas:
"Ægroti hoc capiant pulmenta ex pulvere: peſtem
"Illa domant, ægris ſenſim medicantia fibris."

The poet then treats of its medicinal virtues, and of the employment of it in baking cakes of ſeveral ſorts, with cautions againſt uſing it vainly as a coſmetic. He afterwards deſcribes the different ways of making it into bread, and of eating it; and gives an account, in detail, of the method employed, by the native Indians, to obtain from manioc-root, by chewing it, and ſetting the chewed maſs to ferment in the ſaliva with water, an intoxicating or maddening ſpirit. This leads him into an animated epiſode, too long for inſertion here, of the manner of fatting, killing and eating their captives in war, by the ſavages and cannibals of Brazil, worked up to phrenzy by the drink ſo prepared. Some further deſcription of the manners of the aborigines, and a recital of the recovery of the author from a threatened conſumption of the lungs by the uſe of manioc, concludes this poem.

To ſuch of our readers as wiſh a conciſe hiſtory of caſſava, without the ornaments of poetry, Mr. BRYANT's account of it, in his *Arrangement of Eſculent Plants*, will anſwer the purpoſes of general information.

The *ſecond* poem is on the "Management of cattle." Conſiderably different, indeed, from the part of VIRGIL's Georgics which is employed on the ſame ſubject, both as to the matter and manner of it, is this piece of American verſification. Here are few deſcriptions which are accommodated to the mild ſtate of cultivated Europe. Here are no ſheep and lambs to ſalute the reader with their bleating. In this performance, which is of the *Bucolic* form, the poet deſcribes not a little drove of cows and oxen grazing upon the fields of a ſmall Arcadian or Mantuan farm, but treats of the vaſt woods and ranges of Brazil, of the unnumbered herds of cattle turned out to feed during the ſeaſon, and browze in the wilderneſs; of the method of collecting as many as poſſible of them into pens; of caſtrating, marking, and branding them; and of the deſtruction made among them by the crows (urubuz), bats, panthers, and ſerpents. There is, however, a pleaſant, though ſhort account, of the cultivated country, and paſture lands, about Rio Janeiro (p. 33). We ſelect, as an example of the writer's ſkill, his deſcription of the manner in which cattle are bled to death by a large ſpecies of bat (p. 45).

"Multum etiam infeſtant alati paſcua mures,
"Veſpertina boum peſtis, qua triſtior arvis
"Nulla furit totis, bubulumque haurire cruorem

" Callidior: noctis ſub tempora ſcilicet alis
" Inſtrepit, armentumque petit, morſuque cruentat.
" At quo ſenſum adimat pecori, mira utitur arte.
" Dente bovem pungit, ſimul ore reciprocat auram,
" Atque fovet ſufflando cutem, ſopitque dolorem.
" Saucius haud ſentit bos vulnera, nec fugat hoſtem,
" Neſcius inſideat ſibi quantum in tergore monſtrum.
" Ergo inſtat, largèque cruorem ſugit hirudo
" Aligera, et (ne plena crepet) quantum ebibit ore,
" Tantundem, alternis vicibus, fœdo egerit alvi
" Profluvio, donec ſaturata atque ebria, potu
" Abſtinet: interea vena demiſſus aperta
" It ſanguis ſine lege fluens: tum denique vires
" Deficiunt ægrum, et cum ſanguine vita relinquit."

The cultivation of the tobacco-plant in Brazil is the ſubject of the author's third poem. This is more ſportive and amuſing than either of the former; ſince, after offering his ſentiments on the growth, manufacture, and commerce of tobacco, he treats of the recreation of ſnuffing and ſmoking it with a good deal of humour and pleaſantry. He writes like one who could take a pinch of the one or a whiff of the other with a perfect reliſh. There are, nevertheleſs, ſome ſolemn verſes in this piece, as our readers will judge from the following ſhort extract on the ſlave-trade, wherein tobacco is bartered away for negroes along the coaſts of Africa.

——————— " At Nicotiana vel ipſos
" Glomera veliferis ratibus mittuntur ad Affros;
" Non tamen argenti vi permutanda vel auri,
" Sed certo juvenum numero; et quæ nautica pinus
" Nicotiis onerata glomis de littore ſolvit
" Braziliæ, redit ecce viros, miſeriſque referta
" Mancipiis redit; arva quibus ſudore colenda,
" Vitaque perpetuos inter ducenda labores."

The verſification of theſe pieces is, in ſome inſtances, happy; and, generally, plain and deſcriptive. But the claſſical reader will find a conſiderable number of new words, which neither Maro nor his contemporaries would underſtand. Yet this is not a ſubject of blame; ſince new ideas behoved to be expreſſed in novel words; and we are inclined to think the author has great merit in getting over this difficulty with ſo

much addreſs as we diſcover. Lucretius complained of the poverty of the Roman language, and the want of expreſſions in it ſuitable to ſpeculative and philoſophical diſcuſſions. It is, then, no wonder if the author before us laboured under the preſſure of a heavier inconvenience, when he wrote in the Roman language, upon ſubjects ſo wholly unknown to the Romans as *manioc* and *tobacco*, and the *land* of *Brazil*, in which theſe two plants are cultivated. Still we obſerve ſome heavy lines, which border nearly upon proſe—ſome unclaſſical words and phraſes where the occaſion did not ſeem to require them—and ſome miſtakes of the preſs.

Our limits forbid us to enter into any minute account of the fourth poem in this collection, written by PRUDENTIUS AMARALIUS, *on the cultivation of cane, and the manufacture of ſugar*, as it appears to reſemble, in many reſpects, both as to ſtyle and manner, the pieces which precede it, and to poſſeſs no very prominent features of difference.

ART. II. *Fragments of the Natural Hiſtory of Pennſylvania. By* Benjamin Smith Barton, *M. D. Correſpondent Member of the Society of the Antiquaries of Scotland, &c. &c. Part firſt.* Philadelphia. *Printed for the Author by* Way *and* Groff. 1799. pp. 24. folio. *With an Introduction of* 18 *pages.*

THIS work, which is ſomewhat of a miſcellaneous caſt, is more employed on *ornithology* than on any other branch of natural hiſtory. The author endeavours to add new matter to the ſtock of information concerning the birds of the State in which he reſides, which may be conſidered as applying very nearly to what are called the middle States of the American Confederacy, lying between the rivers Potowmack and Hudſon. The body of the "Fragments" is divided into three ſections—the *firſt* of which treats of the birds which migrate during ſpring and ſummer; the *ſecond*, of thoſe which paſs from region to region in the fall and winter; and the *third*, of the reſident birds of Pennſylvania, or thoſe which abide there throughout the year.

To the regiſter of obſervations on the arrivals of the birds, is added a column of botanical occurrences, after the manner of a *calendarium floræ*; and another of miſcellaneous re-

marks, chiefly Meteorological and Zoological. Theſe comprize the firſt and ſecond ſections, which, we find, were begun to be kept in March, 1791, and end in February, 1792, and are comprehended in five half ſheets. The third ſection contains two half ſheets more. The appendix contains ſome obſervations on the greater number of birds mentioned in the tables.

The migration of birds is a very curious ſubject of natural hiſtory, and has generally been aſcribed, by obſervers, to that power or faculty denominated *inſtinct*. Profeſſor Barton, who has, for many years, been a witneſs of the motions of this claſs of animals, is of a contrary opinion, and oppoſes the notion that birds "never acquire any knowledge by ex-"perience." We are very glad to find him engaged in a work upon *the inſtinct of animals*, which is intended for publication (Introduc. p. 16, note). He promiſes, alſo, a *geographical view of the trees and ſhrubs of North-America* (p. 7); and a *memoir on that deſtructive inſect*, which we believe to be an indigenous ſpecies of tipula, *called the Heſſian-fly* (p. 23): as likewiſe parts *ſecond* and *third* of theſe "Fragments" (p. 24), if this *firſt* number is well received; and *an extenſive work on the vegetables of Pennſylvania and ſome of the adjoining States*; beſides ſeveral eſſays already announced in the courſe of our work, in 1797 (Medical Repoſitory, vol. i. p. 258 and 386), and which have not, as yet, been committed to the preſs, that we have heard of. It muſt be regretted, by all lovers of natural hiſtory, that a "want of leiſure" ſhould prevent the learned author's intention of offering to the world the fruits of his labours on theſe ſubjects, as well as from publiſhing "a num-"ber of tracts relative to the natural hiſtory of North-Ame-"rica, which have long laid in his cloſet." It appears to us that the *fragments*, and the other pieces mentioned, as far as we can judge from their titles, are but chapters, as it were, or ſubdiviſions, of what ought to be one great or general work on American natural hiſtory, and which would be abundantly more intereſting if time could be found for the induſtrious profeſſor to methodize them into a more regular form, or arrange them into a WHOLE. We have no doubt of the ſkill of the artiſt to put together his valuable materials in the ſhape of a ſtructure poſſeſſing both compactneſs and ſymmetry. We earneſtly hope the accompliſhment of this deſign, for which Dr. Barton ſeems to us to poſſeſs talents ſo emi-

nently and ſingularly befitting him. We do not agree with him in calling the matter he has collected "fragmentary rubbiſh." Rubbiſh is old ſtuff of buildings undergoing decay. Dr. Barton's are new materials for a grand edifice.

I. The SPRING and SUMMER MIGRATORY BIRDS of Pennſylvania, which he mentions, are the following:

1. Red-wing oriole (ſwamp black-bird), *(oriolus phœniceus)*.
2. Black-headed fly-catcher (pewe), *(muſcicapa fuſca* of Gmelin's edition of Linnæus Syſtema Naturæ).
3. Red lark *(alauda rubra)*. G.
4. Shore-lark (ſky-lark), *(alauda alpeſtris)*.
5. Little ſparrow (titt, chipping-bird), *(fringilla domeſtica.)* Auctoris.
6. Ferruginous finch (ground-ſparrow, hedge-ſparrow), *(fringilla ferruginea)*. G.
7. White-throated finch *(fringilla albicollis)*. G.
8. Black-throated ſparrow *(fringilla melodia)*.
9. Reed-ſparrow *(paſſer palauſtris* of Bartram).
10. Leaſt finch *(fringilla exilis)*. A.
11. Golden finch *(fringilla triſtis)*.
12. *(Fringilla pinus)*. A.
13. Red-breaſted nuthatch *(ſitta canadenſis)*. G.
14. Golden-wing woodpecker (flecker, or flicker), *(picus auratus)*.
15. Crow black-bird *(gracula quiſcula)*.
16. Cowpen-finch (cowpen-bird), *(fringilla pecoris)*.
17. Blue-bird *(motacilla ſialis)*.
18. Common ſnipe (wood-cock), *(ſcolopax gallinago)*.
19. Noiſy plover (kill-dee, kill-deer), *(charadrius vociferus)*.
20. Little wood-cock (meadow ſnipe), *(ſcolopax minor)*. G.
21. Purple martin *(hirundo purpurea)*.
22. Houſe-ſwallow *(hirundo ruſtica)*.
23. Little bank-martin *(hirundo ——)*.
24. Fiſhing-hawk *(falco piſcatorius)*.
25. Pine-creeper *(certhia pinus)*.
26. Little field-ſparrow *(fringilla graminea)*. G.
27. Golden-crowned warbler *(motacilla coronata)*. G.
28. Cærulean warbler *(motacilla cærulea)*. G.
29. Great blue or aſh-coloured heron *(ardea herodias)*.
30. Belted king-fiſher (king-fiſher), *(alcedo alcyon)*.
31. Greater loon, or diver (loon), *(colymbus ſeptentrionalis)*.

32. Eel-crow *(colymbus migratorius* of Bartram.)
33. Pied-bill grebe (dobchick), *(colymbus podiceps)*.
34. Canada-goose (wild-goose), *(anas canadenſis)*.
35. Buffel-duck *(anas bucephala)*.
36. White-faced teal *(anas diſcors)*.
37. Blue-winged teal *(anas fuſca)*.
38. The blue-bill *(anas ſubcærulea* of Bartram).
39. Mallard-duck *(anas boſchas)*.
40. Fan-creſted-duck *(mergus cucullatus)*.
41. Summer-duck *(anas ſponſa)*.
42. Carolina-pigeon (turtle-dove), *(columba carolinenſis)*.
43. Paſſenger-pigeon (wild-pigeon), *(columba migratoria)*.
44. Grey ſand-piper? (plover), *(tringa ſquatarola?)*.
45. Ferruginous thruſh (thruſh, thraſher) *(turdus rufus)*.
46. Night-heron (qua-bird), *(ardea nyctícorax)*.
47. Marſh-wren *(motacilla troglodytes)*.
48. Green-bittern, or poke *(ardea vireſcens)*.
49. Houſe-wren, ſociable-wren *(certhia familiaris)*. A.
50. Towhee-bunting (towhee-bird, ground-robin, chewink), *(fringilla erythrophthalma)*.
51. Aculeated ſwallow (chimney-bird), *(hirundo pelaſgia)*.
52. Whip-poor-will, or night-hawk *(caprimulgus virginianus)*. G.
53. King-bird, tyrant *(lanius tyrannus)*.
54. Baltimore-oriole, Baltimore-bird (hang-bird), (*oriolus Baltimore*).
55. Baſtard-oriole, baſtard-Baltimore (*oriolus ſpurius*).
56. Cat fly-catcher (cat-bird), (*muſcicapa carolinenſis*).
57. White-poll warbler (*motacilla varia*).
58. Summer yellow-bird (*parus luteus*).
59. Water wag-tail (*motacilla fluvialis*).
60. Warbling-wren, or green-wren (*muſcicapa fuſca*). Cateſby.
61. Red-throated honey-ſucker (humming-bird), (*trochilus colubris*).
62. Yellow-breaſt warbler (Maryland yellow-throat), (*turdus trichas*). G.
63. Red-headed wood-pecker (*picus erytrocephalus*).
64. Golden-crowned-thruſh (*motacilla aurocapilla*).
65. Canada tanager (ſwamp red-bird?), (*tanagra rubra*).
66. (*Muſcicapa ſubfuſca* of Bartram).
67. Red-eyed fly-catcher (*muſcicapa olivacea*).
68. Black-headed warbler (*muſcicapa ruticilla*).

69. Wood-thruſh, little-thruſh (*turdus minor*). G.
70. Brent-gooſe (*anas bernicla*).
71. Creſted fly-catcher (*muſcicapa crinita*).
72. Yellow-breaſted chat (*muſcicapa viridis*). G.
73. Sparrow-hawk (*falco ſparverius*).
74. Indigo-bunting, blue-linnet (*tanagra cyanea*).
75. Cuckoo of Carolina (*cuculus americanus*).
76. Creſcent ſtare (meadow-lark), (*alauda magna*).
77. Creeping titmouſe, finch creeper (*parus americanus*).
78. Hooded titmouſe (*motacilla mitrata*).
79. Black-throat warbler, blue fly-catcher (*motacilla canadenſis*). G.
80. Spotted ſand-piper (*tringa macularia*). G.
81. Golden-winged fly-catcher (*motacilla chryſoptera*).
82. Red-headed warbler (*motacilla petechia*).
83. Green warbler (*motacilla virens*).
84. Bloody-ſide warbler (*motacilla pennſylvanica*).
85. Olive-coloured fly-catcher, or leſſer pewe (*muſcicapa rapax* of Bartram).
86. Marſh-bittern (*ardea cinerea*).
87. Rice-bunting (rice-bird, reed-bird, bob-lincoln?), (*emberiza oryzivora*).
88. Soree gallinule (*rallus carolinus*). G.
89. Virginian rail (*rallus virginianus*).
90. Clapper-rail (meadow-clapper?), (*rallus crepitans*). G.
91. Common coot (*fulica atra*).
92. The little ſtriped bittern (*ardea parva* of Bartram).
93. Calandra-lark (May-bird, graſs-bird), (*alauda calandra*).
94. Worm-eater (*motacilla vermivora*). G.
95. Great yellow-throated-wren of Florida (*certhia floridana*). A.
96. Carrion-vulture (turkey-buzzard), (*vultur aura*).
97. Great white-heron (*ardea alba*).
98. Little white-heron (*ardea aequinoctialis*).
99. Swallow-tailed falcon (*falco furcatus*),
100. Rice-bunting (rice-bird, reed-bird). The females excluſively? make their appearance. (*Emberiza oryzivora*).

II. The AUTUMNAL AND WINTER BIRDS of paſſage in his catalogue are not ſo numerous, viz.

1. (*Parus domeſticus*).
2. Creſted titmouſe (*parus bicolor*)

3. Virginian titmouſe, yellow-rump (*parus virginianus*).
4. Prib-chatterer (cedar-bird), (*ampelis garrulus*).
5. Blue and green wing teal (*anas querquedula*).
6. Black duck (*anas nigra*).
7. Mallard (*anas boſchas*).
8. Virginian titmouſe (*parus virginianus*).
9. Canada-gooſe (*anas canadenſis*).
10. Yellow-bellied wood-pecker (*picus varius*).
11. Golden-crowned wren (*motacilla regulus*).
12. Ruby-crowned warbler (*motacilla calendula*).
13. Cat-bird (*muſcicapa carolinenſis*).
14. Wood-cock (*ſcolopax gallinago*).
15. Brent-gooſe (*anas bernicla*).
16. Snow-bird (*fringilla hudſonias*).
17. Fox-coloured ſparrow (*fringilla ferruginea*).
18. Large brown ſparrow, with red eye-brows (*fringilla albicollis*).
19. Little falcon (*falco ſparverius*).
20. Sharp-winged day-owl (*ſtrix diurnalis*). A.
21. Greater redpoll? (*fringilla cannabina?*).
22. Carolina-pigeon (*columba carolinenſis*).
23. Greater redpoll? (*fringilla cannabina?*).
24. Snow-bunting (*emberiza nivalis*).
25. Tawney-faced owl (*ſtrix ſtridula?*).
26. Great-white-owl (*ſtrix nyctea?*).
27. Shore-lark (ſky-lark), (*alauda alpeſtris*).
28. Red-lark (*alauda rubra*). G.
29. Pine-finch (*fringilla pinus*). A.
30. Ferruginous finch (*fringilla ferruginea*).
31. White-throated finch (*fringilla albicollis*). G.
32. Golden-finch (*fringilla triſtis*).

III. The RESIDENT BIRDS enumerated are:

1. White-headed eagle (bald-eagle), (*falco leucocephalus*).
2. Great grey eagle (*falco regalis* of Bartram).
3. Great red-tailed hawk (*falco aquilinus* of Bartram).
4. Pigeon-hawk (*falco columbarius*).
5. Bluiſh-hawk (*falco glaucus* of Bartram).
6. Great horned owl (*ſtrix virginiana*).
7. Red owl, little owl (ſcreech owl), (*ſtrix aſio*).
8. Hen owl (*ſtrix varius* of Bartram).
9. Great ſhrike (guſt-bird? nine-killer), (*lanius excubitor*).

10. Red-backed ſhrike (*lanius collurio?*).
11. Raven crow (raven), (*corvus corax*).
12. Carrion crow (crow), (*corvus corone*).
13. Blue-crow (blue-jay, jay-bird), (*corvus criſtatus*).
14. Pileated wood-pecker (wood-cock), (*picus pileatus*).
15. Red-headed wood-pecker [*picus erythrocephalus*].
16. Golden-wing wood-pecker [*picuſauratus*].
17. Hairy wood-pecker [*picus villoſus*].
18. Downy wood-pecker [*picus pubeſcens*].
19. Black-headed nuthatch [ſap-ſucker], [*ſitta*].
20. Brown-creeper [*certhia fuſca* of Bartram].
21. Great heron [*ardea herodias*].
22. Noiſy plover [kill-dee, kill-deer], [*charadrius vociferus*].
23. Wild-turkey [*meleagris gallopavo*].
24. Ruffed grous [grous], [*tetrao umbellus*].
25. Pheaſant of Pennſylvania [pheaſant], [*tetrao cupido.*]
26. Maryland partridge [partridge, quail], [*tetrao virginianus*].
27. Paſſenger-pigeon [wild-pigeon], [*columba migratoria*].
28. Carolina-pigeon [turtle-dove], [*columba carolinenſis*].
29. Creſcent ſtare [meadow-lark], [*alauda magna*].
30. Read-breaſted thruſh [robin], [*turdus migratorius*].
31. Mimic thruſh [mocking-bird], [*turdus polyglottos*].
32. Prib chatterer [cedar-bird,] [*ampelis garrulus.*]
33. Cardinal Groſbeak [Virginia nightingale,] [*loxia cardinalis*].
34. Croſs-bill [ſhear-bird], [*loxia curviroſtra*].
35. [Blue-bird], [*motacilla ſialis*].
36. [Marſh-wren], [*motacilla troglodytes?*].
37. Toupet titmouſe [*parus bicolor*].
38. Canada titmouſe, little pied titmouſe [*parus atricapillus*].
39. Virginian titmouſe [*parus virginianus*].

The introduction of the work conſiſts of thirty articles, or paragraphs, full of inſtructive ſentences, the reſult of much patient and diligent inquiry, and which it is not eaſy to abridge or analyze.

As we have in our poſſeſſion a manuſcript of Dr. Mitchill's, kept, during the ſpring of 1787, at Plandome, on Long-Iſland, we ſhall exhibit from it a few comparative remarks of the arrival and departure of ſome migratory birds of the maritime parts of New-York; being aware, at the ſame time, that the migration of birds being *an act of their volition*,

their annual flights are governed by various accidents, and particularly by the greater or leſs forwardneſs of the ſeaſon; and that, therefore, further obſervations will be neceſſary to render the compariſons complete. They will, however, add ſomething to the ſtock of facts.

MIGRATIONS *in the vicinity of Philadelphia*—1791.

April 15. Wild-gooſe (*anas canadenſis*), arrived in Pennſylvania.

March 12. *Gracula quiſcula?* (crow black-bird)—*picus auratus* (golden-wing wood-pecker—*muſcicapa fuſca* (pewe)—*fringilla domeſtica* (chipping-bird)—arrived.

March 15. *Motacilla ſialis* (blue-bird).

April 15. *Alcedo alcyon* (king-fiſher)—*hirundo ruſtica* (houſe-ſwallow)—come.

April 18. *Turdus rufus* (ferruginous thruſh)—*ardea nyctícorax* (night-heron)—make their appearance.

April 20. *Ardea vireſcens* (green bittern or poke)—ſeen.

April 23. *Caprimulgus virginianus* (whip-poor-will, or night-hawk)—*oriolus Baltimore* (hang-bird)—*lanius tyrannus* (king-bird)—and *fringilla erythrophthalmia* (chewink)—come.

MIGRATIONS *in the neighbourhood of Plandome*—1787.

March 16. Wild-geeſe fly away to the northward, from the ſouth ſide of Long-Iſland, where they winter.

March 19. Black-birds, blue-birds, and chipping-birds, arrived from the warmer latitudes.

March 20. *Golden-wing wood-pecker*, or *clape*—*pewe* or *phœbe-bird*—*king-fiſhers* and *houſe-ſwallows*—ſeen. *Robins* ſinging about the houſe (*turdus migratorius*).

April 28. *Brown-thruſhes* and *night-hawks* make their appearance. The *whip-poor-will*, which is conjectured, by ſome, to be the female of the common night-hawk, firſt heard on the 5th of May.

May 1. Night-barkers and ſhite-pokes arrived.

May 2. Hanging-birds, king-birds, and cheurinks, arrived.

As Profeſſor Barton's work is chiefly a record of facts, we recommend it to our readers who are deſirous of more minute information than our limits permit us to give. The utility, however, of the following remarks on *inſects, as the food of birds*, recommends them to us as worthy of being inſerted entire.

"It may, in the firſt place, be obſerved, that inſects appear to be the firſt food of almoſt all the birds of our country. The more I have inquired, the more I have been convinced, that almoſt all birds live, in ſome meaſure, upon inſects. Even thoſe ſpecies which conſume conſiderable quantities of ſeeds, berries, and fruit, alſo conſume large quantities of inſects: and there are reaſons to believe, that others, whoſe principal food is the nectar of plants, alſo live partly upon theſe inſects. Thus Mr. Brandis found the veſtiges of inſects in the ſtomach of the trochilus, or humming-bird, one of the

laſt birds one would have ſuſpected of feeding on animal food.

"The greater number of our ſmaller birds, of the order of *paſſeres*, ſeem to demand our attention and protection. Some of them feed pretty entirely upon inſects, and others upon a mixed food—that is, inſects and the vegetable ſeeds, &c. Many of them contribute much to our pleaſure by the melody of their notes. I believe the injury they do us is but ſmall compared to the good they render us. I ſhall mention, under ſix different heads, a few of the uſeful birds of this and ſome other orders.

"I. *Muſcicapa acadica* of Gmelin? This is the leſſer creſted fly-catcher of Pennant. It is called, in Pennſylvania, the leſſer or wood-pewe. This little bird builds in woods and in foreſts. After the young have left the neſts, the parents conduct them to the gardens and habitations of men. Here the whole brood dwells in trees near the houſes, where they are fed with the common houſe-fly, and other inſects that are caught by the old birds. The young ones are ſoon capable of obtaining their food in the ſame way. This ſpecies of muſcicapa viſits us in the ſpring, and commonly continues with us until late in September, when it retires ſoutherly to winter.

"II. The *motacilla ſialis*, or blue-bird, feeds principally, if not entirely, upon inſects, both ſuch as are flying and ſuch as are reptile. It is ſaid they eat currants.

"III. Moſt of our ſpecies of picus, or wood-pecker, appear to me to be very uſeful in deſtroying inſects, particularly thoſe which injure our foreſt and orchard trees. It is true, theſe birds are ſometimes injurious to us, by eating ſome of our fineſt fruits, particularly our cherries, and, therefore, pains are taken to expel them from our gardens. But they devour vaſt numbers of inſects, particularly ſome of thoſe ſpecies which prove ſo deſtructive to the trunk of the trees, ſuch as the coleopterous inſects, which, perhaps, do as much miſchief as the caterpillars.

"IV. As a devourer of pernicious inſects, one of the moſt uſeful birds with which I am acquainted is the houſe-wren, or *certhia familiaris?* This little bird ſeems peculiarly fond of the ſociety of man, and it muſt be confeſſed, that it is often protected by his intereſted care. From obſerving the uſefulneſs of this bird in deſtroying inſects, it has long been a cuſtom, in many parts of our country, to fix a ſmall

box at the end of a long pole, in gardens, about houſes, &c. as a place for it to build in. In theſe boxes they build and hatch their young. When the young are hatched, the parent birds feed them with a variety of different inſects, particularly ſuch as are injurious in gardens. One of my friends was at the trouble to obſerve the number of times a pair of theſe birds came from their box, and returned with inſects for their young. He found that they did this from forty to ſixty times in an hour; and, in one particular hour, the birds carried food to their young ſeventy-one times. In this buſineſs they were engaged the greater part of the day; ſay twelve hours. Taking the medium, therefore, of fifty times in an hour, it appeared that a ſingle pair of theſe birds took from the cabbage, ſallad, beans, peas, and other vegetables in the garden, at leaſt ſix hundred inſects in the courſe of one day. This calculation proceeds upon the ſuppoſition, that the two birds took each only a ſingle inſect each time. But it is highly probable they often took ſeveral at a time.

"The ſpecies of certhia of which I am ſpeaking generally hatches twice during the courſe of the ſummer. They are very numerous about Philadelphia, and in other parts of the United States.

"The fact juſt related is well calculated to ſhow the importance of attending to the preſervation of ſome of our native birds. The eſculent vegetables of a whole garden may, perhaps, be preſerved from the depredations of different ſpecies of inſects by ten or fifteen pair of theſe ſmall birds: and, independently of this eſſential ſervice, they are an extremely agreeable companion to man; for their note is pleaſing. A gentleman, in the neighbourhood of Philadelphia, thinks he has already reaped much advantage from the ſervices of theſe wrens. About his fruit trees he has placed a number of boxes for their neſts. In theſe boxes they very readily breed, and feed themſelves and their young with the inſects which are ſo deſtructive to the various kinds of fruit trees, and other vegetables.

"V. The ſervices of the ibis, in devouring the reptiles of Egypt, are well known. They procured to this bird a veneration and regard which form an intereſting fact in its hiſtory, and in the hiſtory of human ſuperſtitions. The ſtorks are, perhaps, not leſs uſeful. Pliny tells us, that theſe birds were ſo much regarded for deſtroying ſerpents, that, in Theſſaly, in his age, it was a capital crime to kill them,

and that the puniſhment was the ſame as that for murder. Virgil hints at the uſefulneſs of the ſtork when he deſcribes it as 'longis inviſa colubris.' In Holland, even in our times, they go wild, protected by the government, from a ſenſe of their uſefulneſs in the way I have mentioned.

"In Britain, if it were not for the herons, and ſome other birds of this tribe, the frogs, and toads, and other reptiles, would increaſe to ſo great a degree as to prove a real nuiſance. North-America abounds with birds of this order; and we even have ſome ſpecies of ibis, very nearly allied to the ibis of Egypt—ſuch as the *tantalus loculator*, or wood-pelecan, the *tantalus ruber*, or ſcarlet ibis, the *tantalus fuſcus*, or brown ibis, and the *tantalus albus*, or white ibis. Mr. Bartram informs us, that the firſt of theſe birds feeds 'on ſerpents, 'young alligators, frogs, and other reptiles.' It is commonly ſeen 'near the banks of great rivers, in vaſt marſhes or meadows, 'eſpecially ſuch as are cauſed by inundations, and alſo in the 'vaſt deſerted rice plantations.' This bird, both with regard to his general aſpect, and his manners and habits, may be conſidered as the ibis of America. In the midſt of all their ſuperſtitions, I do not find, however, that the native Americans have ever paid any particular regard to this bird. I cannot learn that any of theſe ſpecies of tantalus have ever been ſeen in Pennſylvania.

"VI. Some of the birds of the vultur-kind are extremely uſeful to man, by deſtroying immenſe quantities of carrion, which ſerve to vitiate the air, and, perhaps, in ſome inſtances, to give riſe to malignant epidemics. The *vultur aura*, or turkey-buzzard of our country, is one of the moſt uſeful of theſe birds. In Virginia it is protected by a law of that State. The Abbé Clavigero ſpeaks of the uſefulneſs of the *cozcaquauhtli*, or king of the zopilots, the *vultur papa* of Linnæus. 'The zopilot,' ſays this writer, 'is a moſt uſeful 'bird to that country (Mexico); for they not only clear the 'fields, but attend the crocodiles, and deſtroy the eggs which 'the females of thoſe dreadful amphibious animals leave in 'the ſand, to be hatched by the heat of the ſun. The de'ſtruction of ſuch a bird ought to be prohibited under ſevere 'penalties.'

"I am ſenſible that theſe few facts, which are thrown together without any regard to order, can be of little uſe, except in as far as they may turn the attention of other perſons, who poſſeſs more leiſure and information than myſelf, to the

ſubject, which is at once curious and important. It appears to me to be a ſubject peculiarly intereſting to my countrymen. Perhaps few parts of the world are more infeſted with noxious inſects than the United States. The greater number of theſe inſects are, I believe, natives of the country, though our partiality to the ſoil which gave us birth has not always allowed us to acknowledge this truth. Thus we give to the Heſſians the honour of introducing among us that moſt pernicious inſect, the Heſſian-fly, which, for ſeveral years, has committed, and ſtill commits, ſuch alarming ravages on ſome of our moſt valuable grains, particularly the wheat and the rye. But this inſect is, undoubtedly, a native of America. How it came to be, for ſo long a time, overlooked, will probably be mentioned in a memoir, concerning this and other noxious inſects, which I hope to publiſh.

" Many of the pernicious inſects of the United States ſeem to be increaſing inſtead of diminiſhing. Some of theſe inſects, which originally confined their ravages to the native or wild vegetables, have ſince begun their depredations upon the foreign vegetables, which are often more agreeable to their palates. Thus the *bruchus piſi*, or pea-fly, is a native, and ſeems originally to have fed, in a great meaſure unnoticed, upon the indigenous vegetables which are allied to the pea: but ſince the introduction of this laſt among us, it is the principal, if not the only, vegetable which ſuffers from the ravages of this inſect. The Heſſian-fly could not originally have inhabited the wheat, the rye, and other ſimilar gramina of this kind, for theſe vegetables are not natives of America. It is now more formidable to us than would be an army of twenty thouſand Heſſians, or of any other twenty thouſand hirelings, ſupplied with all the implements of war. The caterpillar, which has begun its ravages upon the leaves of the Lombardy poplar, that contributes ſo much to beautify our city, is, moſt probably, a native of our woods. It prefers this fine foreigner to the leſs palatable leaves upon which it has been formerly accuſtomed to feed. Other inſtances of this kind might be mentioned. They ſhow how very neceſſary it is to watch the migrations of inſects from the native to the introduced vegetables; and they teach us a truth, not, I think, ſufficiently attended to by naturaliſts, that different kinds of inſects are much leſs confined to vegetables of the ſame ſpecies, or to ſpecies of the ſame genus, than has been commonly imagined. It is certain, that the ſame ſpecies of inſects,

in America, often feeds indiſcriminately, and in ſucceſſion, upon plants of very oppoſite genera, and even of very different natural orders.

"Hitherto too little progreſs has been made among us in the diſcovery of remedies for the great miſchiefs occaſioned by inſects. The ſubject has not been examined with ſufficient attention. It has given place to diſcuſſions and inquiries of very inferior utility; and, I fear, it will not claim all that induſtrious attention which it ſo well merits, until the evil ſhall have ſpread ſtill farther. It is, doubtleſs, difficult, but it is by no means impoſſible, to prevent the ravages of noxious inſects. In this important buſineſs ſomething has already been done in our country. We have diſcovered a method of diminiſhing the depredations of the little bug, called cucumber-fly, which proves ſo deſtructive to the cucurbitaceous vines, particularly thoſe of the cucumber and muſk-melon. By manuring our wheat lands, and thereby increaſing the ſtrength and vigour of the wheat, we have leſſened the evil of the Heſſian-fly. By ſuſpending, to our young apple, and other trees, pieces of tow, impregnated with a mixture of brimſtone and train-oil, we have learned how to frighten away the periodical locuſts (*cicada ſeptemdecim* of Linnæus), which often do ſo much injury to our orchards. The American Philoſophical Society, by calling the attention of the public to the decay of our peach-trees, has brought us to a better acquaintance with the cauſes of this decay, and with the means of preventing it. Inſects are, no doubt, one of theſe cauſes. We have made ſome progreſs in preventing the miſchief of the *bruchus piſi*, or pea-fly, which proves ſo deſtructive to one of the fineſt eſculent vegetables. But all that has yet been done is very little, compared to that which remains to be done. The ſubject is as new as it is important."

ART. III. *Three Lectures upon Animal Life, delivered in the Univerſity of Pennſylvania. By* Benjamin Ruſh, *M. D. &c. Philadelphia.* Dobſon. 1799. 8vo. pp. 84.

THOUGH the phenomena and changes of life are inceſſantly taking place before our eyes and within our bodies, yet they appear to be ſtill very imperfectly known. The elements of the ſcience of animal life have ſcarcely been in-

veſtigated with more ſucceſs than thoſe of intellectual nature. In both the progreſs has been impeded by a multitude of difficulties, which nothing but a cloſe view of the ſubject can enable the inquirer to perceive. The employment of the mind in comparing and diſcriminating ideas, and the exerciſe of the muſcles in moving the limbs, are ſo familiarized by habit as to be often performed without conſciouſneſs of the operation. But if the mind be turned inward upon itſelf, to trace the origin and modes of its functions, or if we narrowly inquire into the cauſe of muſcular action, it will be found that our ſimpleſt faculties are involved in myſtery and darkneſs, and that we are ignorant of nothing more than of ourſelves.

The lectures before us form a part of the fundamental doctrines which are taught by the learned author, in quality of Profeſſor of the Inſtitutes of Medicine in the Univerſity of Pennſylvania. He adopts, and accommodates to his deſign, the leading principles of the ſyſtem of the late Dr. Brown; rejecting, at the ſame time, with great correctneſs of diſcrimination, ſeveral of the raſh opinions maintained by that bold and eccentric theoriſt. We cannot agree with Dr. Ruſh in aſcribing to Dr. Cullen the credit of originally developing what he ſuppoſes to form the foundation of the Brunonian ſyſtem. With all our reverence for the memory of Dr. Cullen, we are unable to find, throughout the whole of his works, any diſtinct indication of thoſe luminous truths which are unfolded in the "Elements" of his contemporary and rival. It might, perhaps, be aſſerted with more juſtice, that Brown's doctrine of ſtimulus was founded originally upon Haller's diſcovery of muſcular irritability; though it muſt ſtill be maintained, that the Brunonian principles are extended far beyond any thing that appears ever to have entered the mind of Haller.

According to our author, the conſtituents of perfect life, in the human body, are *motion*, *ſenſation*, and *thought*, united. The difficulty of defining animal life in ſuch manner as to include all its variations, and alſo to diſtinguiſh it from vegetable life, will readily occur.

Before proceeding to the detail of the ſubject, Dr. R. delivers the three following propoſitions:

"I. Every part of the human body (the nails and hair excepted) is endowed with ſenſibility, or excitability, or with both of them."

"II. The whole human body is ſo formed and connected, that impreſſions made in the healthy ſtate upon one part, excite motion or ſenſation, or both, in every other part of the body."

"III. Life is the EFFECT of certain ſtimuli acting upon the ſenſibility and excitability, which are extended, in different degrees, over every external and internal part of the body."

In the firſt place, Dr. R. conſiders animal life, as it appears in the waking and ſleeping ſtates in a healthy adult; and he afterwards inquires into the modification of its cauſes in the fœtal, infant, youthful, and middle ſtates of life, in certain diſeaſes, in different ſtates of ſociety, in different climates, and in different animals.

According to the third general propoſition, Dr. R. aſcribes all the actions and ſenſations of the body to the operation of ſtimuli acting upon the organs of ſenſe and motion. Theſe ſtimuli he divides into external and internal. The external are light, ſound, odors, air, heat, exerciſe, and the pleaſures of the ſenſes. The internal ſtimuli are food, drinks, chyle, the blood, a certain tenſion of the glands which contain ſecreted liquors, and the exerciſes of the faculties of the mind. Each of theſe is treated of particularly in the order in which they have been mentioned. And an account of the condition of life in different parts of the day concludes the firſt lecture.

In the ſecond lecture Dr. R. obſerves, that "the ſtimuli which have been enumerated, when they act collectively and within certain bounds, produce a healthy waking ſtate. But they do not always act collectively, nor in the determined and regular manner deſcribed. There is, in many ſtates of the ſyſtem, a deficiency of ſome ſtimuli, and, in ſome of its ſtates, an apparent abſence of them all." To account for the continuance of animal life under ſuch circumſtances, Dr. R. lays down two propoſitions.

"I. The healthy actions of the body, in the waking ſtate, conſiſt in a proper degree of excitability and excitement."

"In an exact proportion, and a due relation of both, diffuſed uniformly throughout every part of the body, conſiſts good health. Diſeaſe is the reverſe of this. It depends, *in part*, upon a diſproportion between excitement and excitability, and in a partial diſtribution of each of them."

"II. It is a law of the ſyſtem, that the abſence of one natural ſtimulus is generally ſupplied by the increaſed action of others."

Having ſtated theſe propoſitions, Dr. R. proceeds next to inquire into and explain the different degrees and ſtates of animal life, as variouſly exhibited in ſleep—in the fœtus—in infants—in youth and middle age—in old age—in perſons who are blind, deaf and dumb—in idiots—in perſons under the effect of long faſting—and in perſons ſuppoſed to be dead, from drowning, freezing, and other cauſes.

In the third lecture Dr. R. takes a view of the ſtate of animal life in the different inhabitants of our globe, as varied by the circumſtances of civilization, diet, ſituation and climate. As a ſpecimen of the manner in which this part of the ſubject is treated, we ſelect the following:

"Let us next turn our eyes to the miſerable inhabitants of thoſe eaſtern countries which compoſe the Ottoman empire. Here we behold life in its moſt feeble ſtate, not only from the abſence of phyſical, but of other ſtimuli which operate upon the inhabitants of other parts of the world. Among the poor people of Turkey there is a general deficiency of aliment. Mr. Volney, in his travels, tells us, 'That the diet of the Bedouins ſeldom exceeds ſix ounces a 'day, and that it conſiſts of ſix or ſeven dates ſoaked in 'butter-milk, and afterwards mixed with a little ſweet-milk, 'or curds.' There is, likewiſe, a general deficiency among them of ſtimulus, from the operations of the mental faculties; for ſuch is the deſpotiſm of the government in Turkey, that it weakens not only the underſtanding, but it annihilates all that immenſe ſource of ſtimuli which ariſes from the exerciſe of the domeſtic and public affections. A Turk lives wholly to himſelf. In point of time, he occupies only the moment in which he exiſts; for his futurity, as to life and property, belongs altogether to his maſter. Fear is the reigning principle of his actions, and hope and joy ſeldom add a ſingle pulſation to his heart. Tyranny even impoſes a reſtraint upon the ſtimulus which ariſes from converſation; for 'They ſpeak (ſays Mr. Volney) with a flow feeble voice, 'as if the lungs wanted ſtrength to propel air enough through 'the glottis to form diſtinct articulate ſounds.' The ſame traveller adds, that 'They are ſlow in all their motions, that 'their bodies are ſmall, that they have ſmall evacuations, and 'that their blood is ſo deſtitute of ſeroſity, that nothing but 'the greateſt heat can preſerve its fluidity.' The deficiency of aliment, and the abſence of mental ſtimuli, in theſe people, is ſupplied,

"1. By the heat of their climate;

"2. By their paſſion for muſical ſounds and fine clothes; and,

"3. By their general uſe of coffee and opium.

"The more debilitated the body is, the more forcibly theſe ſtimuli act upon it. Hence, according to Mr. Volney, the Bedouins, whoſe ſlender diet has been mentioned, enjoy good health; for this conſiſts not in ſtrength, but in an exact proportion being kept up between the excitability of the body, and the number and force of the ſtimuli which act upon it."

In purſuance of his plan, the author then goes on to mention certain mental ſtimuli, which act nearly alike in the production of animal life upon the individuals of all the nations in the world. Theſe are, the deſire of life—the love of money—public amuſements—the love of dreſs—novelty—the love of fame—the love of country—and the different religions of the world.

Under the guidance of the ſame principles which conduct the author's inquiries concerning the human ſpecies, he proceeds, in the next place, to examine the cauſes of life in all the different claſſes of animals, which he finds remarkably to obey the ſame laws that govern the human body.

He finds, alſo, additional ſupport to his doctrine of animal life, by adverting to the manner in which life and growth are produced in vegetables. Vegetable life, he maintains, is as much the offſpring of ſtimuli as animal, and ſkill in agriculture conſiſts chiefly in the proper application of theſe.

In order ſtill further to eſtabliſh the principles here laid down, the author contraſts the cauſes of life with the cauſes of death, and particularizes the various modes in which this cataſtrophe of the body is effected by the changes induced by old age or diſeaſes.

The ſubject is concluded by inferences, drawn from the doctrine of animal life being the effect of impreſſions upon the body, which ſhow its extenſive application to medicine, metaphyſics, theology and morals.

Throughout the whole of this performance Dr. R. expreſſes himſelf with a very commendable diffidence, whenever the queſtion ariſes concerning the nature of the vital principle of animals. "Should it be aſked," ſays he, "what is that peculiar organization of matter which enables it to emit life, when acted upon by ſtimuli, I anſwer, I do not know." On this point, and on ſeveral others of a ſimilar kind, our au-

thor confines himſelf to a ſtatement of facts, wiſely concluding that, in the preſent ſtate of our knowledge of this ſubject, little advantage could ariſe from conjecture or hypotheſis. The author of Zoonomia has, indeed, ventured to ſtep upon this uncertain ground, and, with great plauſibility, attributes the contractions of animal fibres, as well in the muſcles as in the organs of ſenſe, to the action of a ſubtile fluid, which he calls *ſenſorial power*, or the *ſpirit of animation*. Before Dr. Darwin, no perſon had ſo formally attempted to inveſtigate the laws and arrange the phenomena of this attenuated fluid, ſuppoſed to produce the actions of animal life. Notwithſtanding all the ingenuity which Dr. Darwin beſtows upon his doctrine of ſenſorial power, there are ſolid objectoins to the aſſumption of ſubtile fluids and of occult qualities, which equally elude conception and experiment. And we conceive that the warmeſt admirers of Zoonomia, whatever degree of probability may be aſſigned to this hypotheſis, muſt admit that it is incapable of abſolute proof.

The brief expoſition of the contents of theſe lectures, which our limits permit us to give, will, we hope, ſerve as an incentive to the reader to peruſe the whole performance. And, we truſt, the author will hereafter be prevailed upon to lay before the public a more complete view of that courſe of inſtruction which he now delivers in the Univerſity of Pennſylvania, ſo greatly to the gratification and improvement of his numerous pupils.

ART. IV. *Occaſional Reflections on the Operation of the Small-Pox; or the Traveller's Pocket-Doctor. By* Joſeph Hamilton, *Phyſician.* Catſkill. *Croſwell.* pp. 45. 12mo. 1799.

THE reaſon of our noticing this ſmall eſſay is not ſo much on account of any thing new, or not generally known, contained in it, on the diſeaſe which the writer profeſſes to conſider, as becauſe we think his remarks on the adminiſtration of the mild muriate of quickſilver (calomel), in fevers worthy of being preſented to our readers. The quotation will exhibit, at once, a ſketch of his manner of thinking, and of expreſſing his thoughts.

" As I have, in this diſcourſe, been pretty liberal in my

recommendations of the uſe of calomel in fevers, and in large doſes; and as the prejudices againſt the uſe of this moſt ineſtimable medicine do ſtill prevail to a conſiderable degree, even among ſome of the moſt learned phyſicians, to the great hindrance, as I think, of the growth of the knowledge of the healing art, I cannot conſent to commit this little work to the preſs, without firſt remarking that, as early as the year 1783, I publiſhed a ſhort diſcourſe on mercury, in which it was then obſerved, that I had been in the uſe of calomel, as my moſt common febrifuge, for the ſpace of about twelve years, and recommended the uſe of it to the world, to be given in doſes, from 8 to 35 grains. The printed pamphlet, by ſome means, fell into the hands of the Society for the Encouragement of Arts, Sciences, &c. at Boſton. That honourable ſociety appeared to view it as a dangerous publication, and appointed a committee to obſerve thereon, whoſe animadverſions appeared by no means to be entirely deſtitute of aſperity. They were tranſmitted to me in a letter from Governor Bowdoin, their then preſident. The committee would not believe the facts, ſtated in my publication, that the uſe of calomel, in large doſes, in fevers, had been ſalutary. There were contained in the *report*, made by the honourable profeſſor of phyſic, theſe words, viz. 'I believe 'that mankind are not yet ſo happy.' Hereupon I found my reputation at ſtake. In order, therefore, to prove thoſe facts, I was at very conſiderable trouble and expenſe to collect, from various parts of the State of Connecticut, numbers of affidavits, taken before the civil magiſtrate and ſealed, which I tranſmitted to the ſociety for their ſatisfaction; upon which I received a complaiſant letter from Mr. Bowdoin, incloſing farther animadverſions on the ſubject, with a promiſe from the honourable profeſſor to try the experiment in Boſton. Mr. Bowdoin alſo ſignified his deſire that I ſhould write farther on the ſubject, and that it might be printed, at the expenſe of the ſociety, in the firſt volume of their Tranſactions, which was then preparing for the preſs. However, having been worried with the oppoſition I had met with, and being, at that time, in a declining ſtate of health, notwithſtanding I conceived the honourable ſociety to have been erected for the expreſs purpoſe of encouraging arts, ſciences, &c. yet, having found it ſo very difficult and expenſive for me to approach them, I have hitherto neglected it; and have, at this time, preferred an addreſs to the people at large, to whom I would

obſerve, that, ever ſince my aforeſaid publication, I have continued in the practice of phyſic, and in the uſe of calomel, as a febrifuge, in the manner deſcribed, and ſtill believe it to be the moſt expeditious, ſafe, and effectual febrifuge that I ever had the knowledge of; and that it is, in its effects, the greateſt antiputreſcent hitherto known: neither ſhould the phyſician fear to give it on account of its diſſolving the juices, and ſo haſtening the putrefaction of the ſame; for it evidently has no ſuch tendency," &c. &c. &c.

The aſſurances the pamphlet contains of the ſafety of exhibiting calomel in very large doſes, from a phyſician who has practiſed ſo long a time as Mr. Hamilton, are certainly well worthy of attention.

We, however, believe the method of inoculating for the ſmall-pox by abſorption through the cuticle is not much practiſed; we, therefore, inſert the author's experiment in his own words:

"In the year 1785 I impregnated a bit of clean ſponge with the infection of the ſmall-pox; and, as I had before experienced that froſt was abſolute deſtruction to the variolous matter, I carefully kept it from the cold until it was perfectly dry; and then, with ſciſſars, I cut from this ſponge a piece as big as the head of a large braſs pin; and, after having cleanſed with the end of my finger, wet with ſpittle, a ſufficient ſpot of the ſkin on the arm of a child aged ſix months, while aſleep in bed, I put thereon my little bit of ſponge, carefully dropping water from the point of a pin, a ſufficiency to moiſten it a little, and then ſecured the ſponge in its place by a plaiſter of ſimple diachylon as big as an Engliſh ſhilling, and ſecured farther with a bandage. This was done at eight o'clock at night. Early the next morning all was thrown off from the arm, and the arm waſhed in cold water. We could plainly diſcover a red and hard tumor, about the bigneſs of the bit of ſponge which lay over it. This almoſt diſappeared at turns; but on the fifth day from the firſt application this tumor became an handſome pock, together with four more—three on the face and one on the arm—making five in the whole."

The violence of the ſubſequent ſymptoms in this caſe are in no wiſe recommendatory of this manner of inoculating.

MEDICAL & PHILOSOPHICAL NEWS.

DOMESTIC.

Pestilence.

OUR ſummer and autumn have not paſſed away without another viſitation of the Yellow Fever in divers parts of the United States. The mildneſs of the ſummer gave hopes of mitigation, if not of immunity; and, happily, theſe have been ſo far realized, that the ravages of this year, in moſt places, have fallen greatly below thoſe of the laſt. In Philadelphia and New-York the diſeaſe began and terminated nearly at the ſame periods. Some caſes were obſerved early in July—in Auguſt the number of them was much increaſed—in September the diſeaſe had attained its greateſt height—and in October it declined ſo faſt as to be nearly extinguiſhed by the firſt of November. In both cities the deſertion of the inhabitants was early and very general, eſpecially in thoſe parts where the diſeaſe began and raged with moſt violence. A great number of thoſe, in particular, who had quitted the town at an advanced period of the epidemic, carried the noxious power with them into the country, and died at their places of refuge. The eaſtern States have chiefly eſcaped this calamity. In Boſton it made a very ſlight and tranſient appearance early in the epidemic ſeaſon. At Hartford (Connecticut) it created a great alarm, and raged for ſome time with a conſiderable mortality; but it was limited to one part of the town—a ſpot of low ground—and, upon the deſertion of this, ceaſed to be propagated. Two of the ſouthern States have been viſited in a more ſerious manner. In North-Carolina, according to our information, the towns of Newbern, Wilmington and Waſhington, have ſuffered in a violent degree. And in Charleſton (S. C.) we learn that the diſeaſe began at an early ſtage of the ſeaſon, and continued, for many weeks, to prevail with great mortality; confining, however, its attacks chiefly to ſtrangers, or ſuch as had recently arrived from the country. We hope to obtain, for a future number,

a more particular account of the appearance of the diſeaſe at theſe places.

For the phenomena of the diſeaſe, as it appeared in New-York, we refer the reader to our account of the epidemic of laſt year. Many ſuppoſe that the malignancy of this epidemic, compared with the preceding, was greatly diminiſhed; and there is no queſtion that the number of ſeizures and of deaths in this city was very far inferior to that of 1798. But in a given number of caſes there is reaſon to doubt whether any abatement of the violence of the diſeaſe, or any diminution of mortality, has actually taken place. For ourſelves, we have no doubt that inſtances, as malignant in their aſpect and as precipitate in their termination as any of the laſt year, have been obſerved in the courſe of the late epidemic.

The recurrence of the Yellow Fever has revived the long conteſted queſtion of *foreign* or *domeſtic* origin. A maſs of teſtimony, however, in proof of domeſtic origin, is now collected from ſo many different and authentic ſources, that we conceive the queſtion will ſoon ceaſe to be agitated, and the warmeſt opponents of that opinion acquieſce in the general conviction. The events of the late ſeaſon have greatly narrowed the controverſy; and many of thoſe who inſiſt upon the importation of the Yellow Fever, when brought to explain the mode of it, are found to have abandoned all ſuch acceptation of the term as we ſuppoſe to lead into error.

The term *importation* is ambiguous and perplexing in its application to diſeaſes. Scarcely an inſtance can be adduced, in all the compaſs of language, where more miſtake has ariſen from the miſunderſtanding and miſapplication of a ſingle word. The mercantile uſe of it is, indeed, well underſtood; and, when employed to expreſs the bringing of articles of merchandize into a country from abroad, in bulk, it ſeems ſcarcely liable to be miſtaken. But a moment's conſideration will ſatisfy us that this ſenſe of the word will not apply to the introduction of the diſeaſe in queſtion. As examples of the ambiguity of this term, in reſpect of the origin of Yellow Fever, we ſhall briefly notice ſome of the various ſenſes in which it has been uſed by different perſons.

1. Is the Yellow Fever imported from the Weſt-Indies by a continued propagation of contagion from one perſon to another in ſucceſſion, or by a lodgment of ſuch contagious matter, originally derived from the bodies of perſons ill of that diſeaſe,

in certain ſubſtances adapted to receive, retain, and afterwards diffuſe it, as in the caſe of ſmall-pox and meaſles? In our opinion this cannot be admitted—becauſe that diſeaſe, according to the beſt authorities, is not contagious in the Weſt-Indies—becauſe it is not pretended to be generally contagious in this country, beyond certain local ranges of air contaminated by other cauſes—becauſe multitudes take the diſeaſe in our cities who never approached the ſick or the veſſels pointed out as the ſource—becauſe multitudes approach the ſick, and perform every requiſite office for them without infection—becauſe the diſeaſe is found to prevail in parts of our country not viſited by perſons from the Weſt-Indies, and inacceſſible to ſea veſſels—becauſe it prevails only in peculiar ſeaſons and ſituations—and becauſe it yields ſpeedily to froſt. It is ſurely wonderful that this *ſpecific contagion* of Yellow Fever contended for, ſo ſubtile as to lurk in the moſt unperceived and unſuſpected places, ſo inextinguiſhable as to reſiſt all the winds and rains of heaven, in the paſſage from the Weſt-Indies, and ſo virulent as ſometimes to cauſe death in a few hours, ſhould yet never produce the prevalence of the diſeaſe in the cities of the United States but juſt at that time when domeſtic miaſmata are exalted by the ſeaſon to the higheſt point of force, and that, preciſely like ſuch miaſmata, it ſhould immediately yield to hard froſt. Theſe circumſtances, as we believe, do not apply to any diſeaſe of ſpecific contagion hitherto known; but they form only a ſmall part of our objections to this mode of importation.

2. Is the Yellow Fever imported into this country by bringing acroſs the ocean a quantity of the peculiar air of the Weſt-Indies, which eſcapes from the holds of veſſels while the cargo is diſcharging at the wharf? This opinion is too abſurd to ſtand in need of refutation. None but an air-tight veſſel could, for ſuch a time, and with ſo much motion, confine a portion of atmoſpherical air received into its hold.

3. Is the Yellow Fever imported by the generation of noxious vapours, ariſing from neglect of cleanlineſs and ventilation, and from the long accumulation of dead animal and vegetable matter, rendered putrid by heat and moiſture, on board the veſſels trading to and from the Weſt-Indies? This mode of importation, if it can with any propriety be called ſo, we believe to be ſometimes a fact. We believe that this diſeaſe is occaſionally taken by receiving the exhalations of ſuch a filthy and putrid ſhip; but we are confident, at the ſame time, that theſe exhalations differ not materially from

ſuch as are emitted from the docks and ſewers, and many other local ſources of putridity which abound in our cities. And we are equally confident that ſuch exhalations from ſhips can never produce an epidemic prevalence of the diſeaſe, without the concurrence and aid of miaſmata from other ſources, for the purpoſe of diffuſing the poiſon. But how is the term *importation* applied to this ſort of noxious power? Is it becauſe it is an accident or contingency which may take place in a ſhip? If a ſhip had never gone to ſea, but had remained at a wharf from the moment of launching, ſhe might ſtill collect a maſs of putrid matters, and thereby become a ſource of diſeaſe. We have no quarrel with the word *importation*, if it be uſed to expreſs this accidental occurrence in ſhips trading to the Weſt-Indies, and if it lead no perſon into error. But we contend that, with the ſame propriety, many other unfortunate accidents occurring at ſea, ſuch as the devaſtation of ſtorms, the damages of cargoes, and the miſeries of famine, might all be denominated *imported* evils.

The advocates of the domeſtic origin of this diſeaſe are often called upon to explain the more frequent occurrence of it within a few years paſt; ſince heat, filth, and putridity, for a long time, and, in ſome inſtances, more remarkably in former times than lately, have characterized the cities of America. But only a moment's reflection will be requiſite to perceive, that the doctrine of importation will by no means relieve this difficulty. As the Yellow Fever has conſtantly, more or leſs, prevailed in the Weſt-Indies, among Europeans and others newly arrived from the higher latitudes, for more than a century paſt, and as our intercourſe with theſe iſlands has been ſubject to little interruption within that period, why has it not been imported, and rendered epidemic every year? In our opinion this difficulty can only be reſolved by aſcribing the malignancy of recent epidemics to a peculiar, though unknown, morbid conſtitution of the atmoſphere; an occurrence which the hiſtory of diſeaſes, (independently of Yellow Fever), in all ages, and in all regions of the globe, is found incontrovertibly to atteſt, and which is obſerved to return and prevail at unequal and indefinite periods.

Of the Nitrous Fumigation.

A book of evidence, on the effect of nitrous vapour in preventing and deſtroying contagion, has been publiſhed

by Mr. Dobſon, in Philadelphia. The principal part of the firſt ninety pages of the work is a republication, with ſome alterations, of pieces contained in the author's work on the Jail-Diſtemper. Our readers will recur to the remarks on the Wincheſter experiment with this gas, and on thoſe made on board the Union hoſpital-ſhip, and in the Ruſſian ſhips, publiſhed in our vol. ii. p. 229, & ſeq. It was there ſtated that the experiments were very inconcluſive: *ventilation, cleanlineſs, better accommodation for the ſick, and a variety of other excellent regulations having been inforced*, and, by a curious kind of logic, the effect of theſe wholeſome precautions being aſcribed ſolely to the nitrous vapour. The body of the volume conſiſts of letters, and extracts of letters, chiefly from ſurgeons in the Britiſh navy, concerning their trials with nitrous vapour in priſons, hoſpitals, and on board of ſhips. The evidence is much of the ſame kind with the former, and in no reſpect more weighty or more to the point. The experimenters ſtill attend ſcrupulouſly to ventilation, cleanlineſs, &c. while their acid fumigations are going on, and then moſt incoherently conclude that the nitrous vapour did all the good. Mr. David Patterſon's long panegyric on this *moſt convenient, moſt elegant, moſt ingenious*, and *moſt efficacious remedy*, as he calls it, contains two or three pages of directions on the utility of *ventilation, changing of wards*, and *cleanlineſs*, practiſed at Forton (p. 108. Phil. edit.) Mr. M'Grigor moved the ſoldiers from an unhealthy to a dry and healthy ſpot (p. 121), and thinned the different rooms of their ſick (p. 123 and 128). Mr. John Griffen ſtates (p. 139), that, *with a due obſervance of cleanlineſs* added to the fumigation, they were enabled to report, after a while, that the jail fever exiſted no longer in the royal hoſpital at Haſlar. Mr. John Snipe teſtifies, that, at Yarmouth, their ſucceſs in ſtopping what he calls a contagious fever, was owing to *cleanlineſs, free ventilation*, and a daily diffuſion of the nitrous gas (p. 149). Captain Lane removed the people from *the ſhip he mentions for a few days, white-waſhed the decks*, and uſed the nitrous fumigation (p. 155). And Mr. R. Cinnamond, and Mr. Robert Sabine, aided the proceſs *by proper attention to cleanlineſs, and by airing between the decks of the ſhip Melampus* (p. 162 and 163). Moſt of the other accounts ſeem mutilated and imperfect. If the relators had mentioned *the whole* of the circumſtances, it is probable that cleanlineſs and

ventilation would have been mentioned in all the trials wherein the fumigation did no harm. What do all theſe experiments prove? Merely this—that the removal of naſtineſs and nuiſances from the perſons, beds, clothing, apartments, and neighbourhood of the ſick, and the admiſſion of a plenty of good reſpirable air, will deſtroy febrile poiſon, *if aſſiſted by nitrous fumigation*—AND SO THEY WILL MOST PERFECTLY WITHOUT IT. This publication is, indeed, full of inſtruction; but the facts it contains require an interpretation very, very different from that which the humane and public-ſpirited author derives from them. It is a remarkable ſpecimen of looſe narrative, and of concluſions not warranted by the premiſes. The way to try the nitrous fumigation fairly is to apply it, and it alone; and this mode we recommend to the advocates of that practice as the only unequivocal means of determining its efficacy. Can any body think it philoſophical and fair, after water, ſoap, lime, ley, and oxygenous air have been freely employed, to aſcribe the good wrought *to the ſalt-petre vapour?*

ADAMANTINE SPAR AND BASALTES.

Dr. Adam Seybert, of Philadelphia, has diſcovered, at Cheſnut-Hill, nine miles from that city, cryſtals of ADAMANTINE SPAR. It is bedded in large maſſes of granite, or rather forms a part of that rock, together with fleſh-coloured feldſpath, ſmoky quartz, greeniſh mica, ſtriated cryſtals of black ſhoerl, and ſometimes ſmall garnets. Its external appearance is very ſimilar to the Bombay ſpecimens; but differs from them in being free from pyrites, and in being much more regularly figured. Its colour is light green, and its hardneſs ſuch as to cut glaſs very readily. Some pieces have the luſtre of glaſs, while others are not ſo bright. In other reſpects the character of it agrees with that given in 1 Kirwan's Mineralogy, p. 335, var. i. From having found other cryſtals of adamantine ſpar among the fragments of granite thrown out by the labourers in digging a reſervoir for water at Philadelphia, the vigilant diſcoverer is led to believe it may be found in many parts of the United States. We are happy to underſtand Dr. Seybert intends to favour the public with a ſet of correct experiments on the compoſition of this curious production of nature.

The ſame gentleman has alſo diſcovered ſome elegant baſaltes, of a very regular form, in a ſituation confirming its

Neptunian origin. It was found in a bed of gravel with breccia, at the upper end of Flour-Town, thirteen miles from Philadelphia.

New Material for the Manufacture of Paper.

Robert R. Livingſton, Preſident of the Agricultural Society, and Chancellor of the State of New-York, has diſcovered a property, in a ſpecies of *conferva* (we believe *conferva rivularis*, Lin.), very plentiful in the water of the Hudſon, to afford excellent paper. From ſamples of paper manufactured from this plant, with an admixture of one ninth of coarſe rags, it ſeems well adapted for wrappings, for hangings, and for book-binders' uſe. As far as can be judged, the cheapneſs and plenty of the material will render this diſcovery very important, by leſſening the price of an article ſo variouſly employed as paper is. It could, doubtleſs, be bleached as well as other paper, either by the common proceſs employed for rags, or by aid of the oxygenated muriatic acid gas; and we have ſeen a ſpecimen in which this has been attempted with conſiderable ſucceſs. The liberal and patriotic inventor will, we hope, purſue the ſubject further.

Chart of Lower-Canada.

There will probably be an addition, ere long, to the ſtock of American Geography. We underſtand that Meſſrs. Vondenvelden and Charland, of the province of Lower Canada, have iſſued propoſals for publiſhing, by ſubſcription, a map, in three ſheets, and a topography, in an octavo volume, of Lower-Canada, including, as well the grants made by the French crown, as the townſhips ſurveyed and laid out by the Britiſh government. In compiling this work, the publiſhers, beſides their own ſurveys, have availed themſelves of the labours of Meſſrs. Gale and Duberger, for the materials from the ſouth-weſtern boundary of the province down to the counties of Quebec and Dorcheſter. It is to be expected, that a work which promiſes ſuch a valuable addition to the topography and hiſtory of America will meet with due encouragement from the public.

Medical Advice from Marseilles.

Timothy Pickering, Secretary of State to the American government, informs Dr. Mitchill, in his letter of October 14,

1799, that he has received from Mr. Cathalan, the conſul of the United States at Marſeilles, ſeveral copies of a pamphlet, compiled there, on the ſubject of the Yellow Fever of America. This collection of pieces was undertaken by Mr. C. in conſequence of a requeſt by the Secretary of State to be informed concerning the inſtitution of the famous lazaretto of Marſeilles. From peruſing a copy of the Conſul's publication, which was politely forwarded to us by the Secretary, we learn very little about the object of Mr. P.'s inquiry; but, on the contrary, we find an opinion of the board of health at Marſeilles, another of ſeveral phyſicians of the ſame place, and a third of a number of medical men at Montpellier, delivered in a very formal manner, on the prevention and cure of the Yellow Fever of America, and on the method of deſtroying its contagion! They are apprehenſive it may be imported at ſome future day from New-York and Philadelphia into the ports of the Mediterranean.

The conſultation of Montpellier is ſubſcribed by Tandon, Fouquet, Chreſtien, and Fagès. Among other preventative means, they recommend to the people of the United States *to eat as little ſalted proviſion as poſſible, to drink tea ſparingly, to diſcontinue entirely the uſe of coffee*, and *to eat no butter nor fat fiſh, ſuch as ſturgeon.* Their curative directions ſeem generally judicious, though mercury is omitted in their liſt of remedies—conſiſting of moderate emetics, cathartics, and ſudorifics, with occaſional blood-letting, freſh air, cleanlineſs, and a light vegetable diet. Our readers will ſmile to learn that they propoſe to deſtroy the contagion *by the burning of caſcarilla and ſpruce* (they perhaps mean *pine) wood*, and to prevent the operation of it *by the ſmoking of tobacco* and *the application of cauteries.* They further adviſe, for the ſame purpoſe, the burning of *aromatic woods* in the ſtreets during the day, and particularly at evening, and the conſtruction *of barracks for the ſick of pine (ſpruce) timber.* The phyſicians Moulard, Joyeuſe, Vidal, and Bouge, of Marſeilles, adviſe temperance, moderate diet, an open belly, chamber-bathing, riding on horſeback, abſtaining from pork, &c. as means of prevention; aceſcents, laxatives, emetics, antiſpaſmodics, antiſeptics, with cupping, blood-letting, and bliſters, according to circumſtances, as means of cure, and refer to the regulations of their lazaretto for information as to the method of ſtopping the contagion, which they ſeem to think are effectual there, and, of courſe,

muſt be infallible in all other places. Though this pamphlet contains little either to amuſe or inſtruct the American phyſician or legiſlator, we have thought it worth the while to give this abſtract of it in juſtice to Mr. Cathalan, of whom the Secretary of State writes in theſe honourable terms: "I am ſure his zeal has been thus diſplayed with the beſt intentions; for he has given uniform proofs of benevolence, and of attention to the intereſts of the United States."

Bread-fruit Tree.

A flouriſhing young bread-fruit tree was, in Auguſt laſt, preſented to Mr. John Wood, of the city of New-York. It was brought from the Iſland of Jamaica by Captain Stephen Clay. Though the rigour of our winters may prevent the growth of this and ſome other plants of the South-Sea Iſlands in the open air, yet they are juſtly eſteemed a valuable acquiſition to the collection of green-houſe plants.

Liberal Decree *of the* Trustees *of* Columbia College, *in reſpect to* Chemistry.

Notwithſtanding it has been ſo long known that natural philoſophy, or the ſcience of *experimental phyſics*, is divided into two great branches, the *mechanical* and the *chemical*, ſtill the *former*, which only treats of the more obvious and ſenſible properties of matter, has been taught in colleges and univerſities. The *latter*, which is employed in aſcertaining the laws which govern the compoſition and decompoſition of material bodies, and ſcrutinizing more nearly the relations and affinities of their component atoms, has rarely or never entered the plan of what is termed a genteel or liberal education; but has been improperly conſidered as auxiliary only to the medical profeſſion. The truſtees of Columbia College have wiſely corrected this error, by determining, at one of their late meetings, that the ſtudy of the *chemical branch of phyſics* ſhould precede the conferring of the degree of Bachelor of Arts upon the ſtudents of that ſeminary: of courſe, the youth educated there will have the advantage of becoming acquainted not only with natural philoſophy, as it is commonly termed, but with chemiſtry. This is an example highly worthy the imitation of other places of public inſtruction.

Additional Experiments, by Dr. RICKETSON, *on the Cultivation of the* POPPY-PLANT, *and the Method of procuring Opium.* (See vol. i. No. 4. p. 420. Appendix).

The intent of the preſent remarks is to illuſtrate my former experiments, and to decide ſome points I then delivered with ambiguity.

In my eſſay, or former experiments, I ſaid—"From the trials I have made, I am of the opinion, that it is a matter of indifference which ſpecies or variety of the plant is cultivated for medicinal uſe, as they all afford, when tapped, a juice that is ſimilar, as to quantity, colour, and every other reſpect, both freſh and when dried: however, I have thought that the large double ſpecies produces the greateſt number of heads, and, conſequently, the largeſt quantity of juice from one ſeed; but of this I have not yet had ſufficient trials to be certain."

In confirmation of which, I yet think that the juice of every kind of the plant yields the ſame opium: but I have lately found one ſpecies or variety of the plant that claims particular attention, and a ſuperiority to all others I have ever ſeen. This is of the ſingle ſpecies, and of the variety of large red or purple flowers. It grows larger and more luxuriant than any other I ever ſaw, that is, from four to five feet in height. It generally produces from two to four ſtalks or heads from one ſeed. The capſules, or heads, grow particularly large, as do alſo the ſtalks: and I muſt here remark, and it is ſufficiently obvious, that it is the two laſt particulars wherein the ſuperiority of this variety of the plant conſiſts.

I have thought that this was longer in arriving to maturity than the common kinds: and as it rarely or never has more than four ſtalks, the ſeeds may be planted as near as ſix or eight inches diſtance.

In my eſſay I ſaid—"The ſtates of the plants, wherein I have found them to yield moſt juice, are juſt before, in the time of, and immediately after flowering."

I now find that they yield moſt plentifully a few days after the flowers have fallen: and if it be collected from the heads in the Aſiatic manner, not until eight, ten, or twelve days after, which will be further known by the heads (particularly the large kind above-mentioned) turning to a red or purple colour; when they will be found to have nearly acquired their full ſize, and to yield moſt plentifully a thick, rich, milky juice.

Agreeable to the reſult of my former trials, I ſaid—"I have tried the Aſiatic method, with ſeveral others; but none have ever ſucceeded ſo well with me as, in a ſunſhining day, to cut off the ſtalks at about an inch diſtance from their flowers, or capſules, and, as ſoon as the juice appears, which it does, at firſt, equally well on that part of the ſtalk cut off with the capſule, or flower, as on the ſtanding part, to collect it with a ſmall ſcoop or penknife, the laſt of which I have found to anſwer the purpoſe very well. After the juice ceaſes to appear on the top of the ſtanding ſtalk, it ſhould be cut off about an inch lower, when it will be found to yield almoſt as freely as before, and repeated as long as any juice appears."

Since which, as it had been ſuggeſted that the method I recommended was not equal to the Aſiatic, I made a number of experiments, to determine the matter with greater certainty: but what is neceſſary to add, and which is ſufficient for the preſent purpoſe, is, that in every experiment, made under equal circumſtances, I obtained the largeſt quantity in the former way, that is, by cutting the ſtalk, though it may require rather more care and labour.

I might have remarked, in my former directions, that the juice will harden ſufficiently by ſtanding in the open air; but as this is ſlow and tedious, the ſun's heat may be always employed.

With reſpect to the comparative ſtrength of the Aſiatic and American opium, all the trials I have ſince made, as well as thoſe of my friends, lead to the ſame concluſion as in my former eſſay.

CHEMICAL NEWS; *communicated in a Letter to* SAMUEL L. MITCHILL, *M. D. Profeſſor of Chemiſtry in Columbia College, New-York, by* JAMES WOODHOUSE, *M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, &c. dated Philadelphia, Auguſt* 22, 1799.

FIRST.

Of the non-Action of the Nitric Acid on Silver, Copper and Tin.

Having occaſion to make a ſolution of ſilver in the nitric acid, ſeveral thin pieces of ſilver were digeſted forty-eight

hours, in a ſmall quantity of the moſt pure and concentrated acid, prepared by diſtilling ſtrong ſulphuric acid on nitre, from which the water of chryſtallization had been thrown off by means of heat, and the metal was not diſſolved. The temperature of the air varied between 75 and 90 degrees of Fahrenheit's thermometer.

This phenomenon was contrary to what ought to have taken place, according to the chemiſts of all nations, who declare that the nitric acid diſſolves ſilver with the utmoſt rapidity.

Suppoſing that the non-action of the acid was owing to the metal being in ſmall maſſes, the filings of ſilver were tried, but no ſolution took place in the ſpace of two days. Having then added a ſmall quantity of water to the acid, the ſilver was diſſolved in a few minutes.

Repeating this experiment upon copper, the ſame effect happened.

Nitric acid was poured upon copper, and no action was produced; but, upon the addition of water, ſolution immediately commenced, and oxygenous and nitrous air was diſcharged; the latter holding a portion of the copper in ſolution, as appeared by immerſing a lighted taper in the nitrous acid gas, when it burned with an enlarged vivid and blue flame. The flame of the taper was frequently blown out, and rekindled by dipping it into the air.

Some concentrated nitric acid was alſo poured upon tin foil, when it remained in a quieſcent ſtate for the ſpace of one week; but, upon the addition of water, the whole was inſtantaneouſly converted into a white oxyd, with the production of a high degree of heat.

The errors of chemiſts, in regard to the action of nitric acid upon tin, will be ſeen more clearly by extracting what has been ſaid upon the ſubject.

Chaptal tells us the nitric acid devours tin, that the decompoſition is ſpeedy, and that the metal is inſtantly precipitated in the form of a white oxyd. The ſame author ſays, Mr. Baumé even pretends that the nitric acid does not diſſolve tin; but Kunckel and the famous Rouelle have maintained the contrary.

Fourcroy declares that tin decompoſes nitric acid, even in the cold, with amazing rapidity, and that this is one of the moſt aſtoniſhingly rapid ſolutions in all chemiſtry.

From what has been ſaid, it appears that Mr. Baumé is

right, and that Fourcroy, Chaptal, Rouelle, and Kunckel, uſed an acid diluted with water.

In what manner does water act in theſe experiments?

Dr. Prieſtley ſuppoſes that no air can be produced without water, and that it is neceſſary to the conſtitution of every kind of air; but this throws little light upon the ſubject, and it does not account for the manner in which water acts in promoting the ſolution of ſilver, copper, and tin, in the nitric acid; and nitrous air may be obtained from zinc and biſmuth by the acid, however concentrated.

It may be ſuppoſed, that the water merely produces heat by uniting with the acid, and ſo diſſolves the metals; but this is not the caſe; for if the acid is diluted with water, and ſtands until it is cool, it will ſpeedily diſſolve them.

It is a common thing with the teachers of chemiſtry to fold up a portion of the dry nitrate of copper in tin foil, and to let it remain, for ſome time, in contact with the tin, to ſhow that it will not act upon the metal in a dry ſtate. The tin foil is then unfolded, and a little water is added to the nitrate of copper, and it is again encloſed in the tin, when a violent action enſues, accompanied with ſparks of fire, and a diſcharge of nitrous air.

The intention of this experiment is to ſhow that bodies do not act upon each other in a dry ſtate—*corpora non agunt niſi ſoluta.* But from the experiments which have been related, of the non-action of the nitric acid on tin, the explanation of what takes place muſt be ſought for in the action of the water on the nitric acid of the nitrate of copper.

Some writers have taken notice of the production of ammoniac, when nitrous acid is added to copper and tin. As the concentrated acid has no action on theſe metals, the ammoniac muſt be produced by the hydrogen of the water uniting with the azote of the nitric acid, while its oxygen, and that of the water, unites to the tin and copper, and converts them into oxyds.

Having related theſe facts, the language of chemiſts, in future, ought to be—The nitric acid has no action on ſilver, copper, and tin; but if water be added to the acid, ſolution ſpeedily takes place.

Dr. Hope has taken notice of the non-action of the nitric acid on ſtrontian earth; and Mr. Leonhardi tells us, that it quickly deſtroys wool and ſilk, but that linen may remain immerſed in a bottle of the ſtrong acid a whole day without injury.

SECONDLY.

Of the Difference in the Quantity of Ammoniac obtained from Bones, by diſtilling with and without a Lute.

A quantity of the bones of horſes and cows were placed in a diſtilling apparatus, formed of iron, which communicated with the worm of a refrigeratory, to which a large glaſs receiver was annexed. Upon applying a high degree of heat, three ounces of volatile alkaline ſpirit, impregnated with the black animal oil of Dippel, were obtained in three hours. The receiver was cloſely luted to the worm, and the air in it was perfectly tranſparent. Upon taking away a part of the lute, in ſuch a manner as to permit the air of the atmoſphere to enter the receiver, it became immediately filled with a thick brown yellow cloud of ſmoke.

Having made a variety of comparative experiments, to determine the difference in the quantity of the product, by diſtilling with and without the lute, it was found that five times as much of the volatile alkaline ſpirit could be obtained by carrying on the diſtillation without the lute, as could be procured, in the ſame ſpace of time, with the application of the lute.

Lavoiſier ſuppoſes, that when ammoniac is obtained from animal ſubſtances, the hydrogen and the azote of theſe bodies unite together, and form the volatile alkali; but it appears, from what has been ſaid, that the azotic air of the atmoſphere enters into the worm of the refrigeratory, joins the hydrogen of the bones, and ſo forms the ammoniac.

Manufacturers of the volatile ſpirit of ſal ammoniac may take ſome valuable hints from theſe experiments.

THIRDLY.

Of putrid Urine expoſed to the Froſt.

A quart of the moſt putrid urine, and of as yellow a colour as gamboge, was expoſed, two nights, to intenſe cold, when it became perfectly ſweet, and was as colourleſs as rock-water.

May not this wonderful change be attributed to the agency of the oxygen gas of the cold atmoſpheric air?

The acid of citrons not only neutralizes the volatile alkali of putrid ſubſtances, but completely deſtroys the nauſeous ſmell which exiſts, independent of the ammoniac. The ſulphuric and muriatic acids have no ſuch effect. Does the

oxygen of the citric acid act here likewiſe? Lowitz, a Ruſſian chemiſt, ſuppoſes that charcoal neutralizes the putrid effluvia of animal bodies; but, in my opinion, it acts mechanically, in preventing the putrid particles of matter from flying into the air.

FOURTHLY.

Of Starch prepared from the Fruit of the Æſculus Pavia, or Horſe Cheſnut.

In the 29th number of the Repertory of Arts, there is an account of a patent, obtained by Lord William Murray, for making ſtarch from the fruit of the æſculus hippocaſtanum. A writer in the London Monthly Magazine for 1798, ſays he has repeatedly, and in various ways, endeavoured to make ſtarch of the fruit, but always unſucceſsfully; for it turns to a yellow colour.

The fruit of our æſculus pavia is much larger than that of the æſculus hippocaſtanum, and is of a white colour: that of the hippocaſtanum is yellow.

A ſingle nut, dried, weighed half an ounce and twenty-five grains, and yielded forty-four grains of fine ſtarch.

I prepared half a pound of this ſtarch from the nuts of the æſculus pavia, and have kept it two years, and the white colour is no way impaired. It is ſuperior to the fineſt Poland ſtarch, and has been uſed, by ſeveral ladies, to ſtarch various articles of dreſs, without imparting any yellow colour to them.

The method of preparing it is, to take off the ſhells from the nuts with a knife; grate them in a veſſel of water which will hold the fine particles of the ſtarch ſuſpended, when they are to be decanted into another veſſel, which muſt remain at reſt until the ſtarch ſubſides to the bottom. The water is then to be poured off, and freſh is to be added, and the ſtarch is to be well ſtirred about in it, when it muſt be again permitted to ſubſide. The water is then to be thrown away, and the ſtarch is to be dried in the ſun.

The water of the firſt waſhing holds a poiſonous matter in ſolution, which, when evaporated to the conſiſtence of an extract, and mixed with dough, will intoxicate and ſwell the bellies of ſmall fiſhes.

FIFTHLY.

Of the Compoſition of Flores Martiales and Ens Veneris.

The French chemiſts ſay that the flores martiales are com-

poſed of ſal ammoniac, coloured by an oxyd of iron, and that ens veneris is ſal ammoniac, coloured by an oxyd of copper; but I can eaſily prove that the one conſiſts of ſal ammoniac and the muriate of iron, and the other of ſal ammoniac and the muriate of copper.

1ſt. When iron or copper is added to ſal ammoniac, the ſalt is decompoſed, the iron or copper unites to the muriatic acid, and forms a muriate of iron or copper, which, when expoſed to heat in cloſe veſſels, ſublimes with the ſal ammoniac, while the volatile alkali is ſet at liberty.

2dly. Flores martiales, or ens veneris, may be made by ſubliming ſal ammoniac with the muriate of iron or copper, prepared by adding the muriatic acid to theſe metals.

3dly. When flores martiales, or ens veneris, are diſſolved in water, and ſuffered to chryſtallize, the muriate of iron and copper will be obtained ſeparate from the ſal ammoniac.

SIXTHLY.

Of Aromatic Oils, obtained from the Pellicle which envelopes the Seeds of the Laurus Saſſafras and Laurus Benzoin.

The method of obtaining theſe oils is to boil the pellicle which ſurrounds the ſeeds of the ſaſſafras and Benjamin-tree in water, when they float upon its ſurface, from which they may be ſkimmed with a ſpoon.

That of the ſaſſafras differs materially from the oil obtained from the bark of the root of this tree. Its aroma is different; it is much lighter, and it congeals in a higher degree of heat.

The oil of the benzoin-tree is a delightful aromatic, is very inflammable, and might be uſed as a ſpice in food, and in all thoſe diſeaſes in which the aromatic oils are uſeful. It has been tried with ſucceſs, as an external application, in a ſevere caſe of chronic rheumatiſm. One half pound of the pellicle of the ſeeds will yield ſeveral ounce meaſures of oil.

SEVENTHLY.

Of the Eudiometer.

The eudiometer is an uſeleſs inſtrument in aſcertaining the purity of atmoſpheric air.

1ſt. Becauſe nitrous air can never be obtained of the ſame degree of ſtrength.

2dly. When one meaſure of nitrous air is added to one meaſure of atmoſpheric air, the abſorption will be great or ſmall, according to the time the air remains over the water, or is agitated in it.

Having made ſeven trials with this inſtrument, with the ſame atmoſpheric air, I obtained a diminution, at the firſt experiment, of

	0	60
2d,	0	56
3d,	0	95
4th,	0	87
5th,	0	90
6th,	0	93
7th,	0	100

From this it is evident, that Dr. Davidſon was deceived in ſuppoſing that the air of Martinique was much purer than the air of Europe, and that the error lay in his inſtrument. The nitrous air which I uſed was procured from nitric acid diluted with water and copper.

EIGHTLY.

Of American Bliſtering Flies.

I have diſcovered two other bliſtering meloes beſides that deſcribed in the Medical Repoſitory. The one I would call MELOE CLEMATIDIS, as it is particularly fond of ſeveral ſpecies of this plant. It is larger than the one deſcribed by Dr. Chapman, and the female is nearly twice the ſize of the male. The head, thorax, elytra, and antennæ, are black; the elytra only edged with white. The abdomen is of a light aſh-colour. The upper part of the abdomen, under the wings, is marked by two longitudinal ſtreaks of a bright clay-colour. The *aſters* are ſometimes black with theſe flies, and the leaves are entirely deſtroyed by them.

The other I would call *meloe nigra*, the *Pennſylvanica* of Linnæus. It is not more than half the ſize of Chapman's fly. The whole of it is black. It feeds upon the prunella vulgaris, or ſelf-heal, and ambroſia trifida, or ſtick weed.

I applied a ſmall bliſter of theſe flies to my ſkin, and loſt the plaiſter in half an hour. In twelve hours after a fine bliſter was produced. A watery extract of the flies bliſtered in ſix hours. Diſtilled in a retort, they yield an acid, whoſe properties have not yet been examined.

Beſides theſe three kinds of meloe, there is another found in this country, mentioned by Kalm, and called by Linnæus *meloe majalis*; but it is not yet known whether it will bliſter; for Shoepf expreſſly aſks the queſtion, "An mel. veſicatorio (cantharid. officinal.) ſubſtituendus?"

We then know for certainty of three kinds of indigenous bliſtering flies—meloe Chapmani, meloe clematidis, and meloe nigra. Meloe majalis, doubtful.

NINTHLY.

Of the Baſe of the Muriatic Acid.

Mr. William Lambe, in an eſſay in the fifth volume of the Mancheſter Memoirs, has attempted to prove that ſulphurated hydrogen is the baſe of the muriatic acid. He obtained ox. muriatic gas by dropping ſulphuric acid upon the reſiduum left after evaporating water which had been impregnated with hepatic gas, in which iron and manganeſe had been digeſted. I have performed this experiment, and the reſult is exactly as ſtated by Mr. Lambe.

Two drachms of the filings of bar-iron were placed in twenty-two ounce meaſures of diſtilled water, which had been impregnated with ſulphurated hydrogen gas, in Nooth's apparatus. In five days twelve ounce meaſures of inflammable air eſcaped from the water. Six ounces of the clear fluid evaporated to dryneſs, left a reſiduum, conſiſting of dephlogiſticated muriate of iron, which attracted the moiſture of the atmoſphere. A few drops of ſulphuric acid, let fall upon it, produced an effervescence, and white clouds of ox. muriatic gas eſcaped, as was very evident from the ſmell, and from the teſts generally uſed to detect the preſence of this gas.

The diſcovery of Mr. Lambe is the beſt which has been made for many years.

A number of literary gentlemen in Connecticut, have lately aſſociated for the purpoſe of encouraging philoſophical reſearches, and, particularly, for developing the natural hiſtory of that State. The aſſociation has aſſumed the title of *The Connecticut Academy of Arts and Sciences.* The general meeting of the Academy, for the election of officers, is to be held, annually, at New-Haven, on the fourth Tueſday of October; and the other meetings are to be on the fourth Tueſdays of December, February, April, June, and Auguſt, at New-Haven.

The following are the officers elected for the preſent year:
Rev. Timothy Dwight, D. D. Preſident.
His Excel. Gov. Trumbull, Vice-Preſident.
Hon. John Treadwell,
Rev. James Dana, D. D.
Dr. Eneas Munſon,
Rev. Bela Hubbard,
Hon. Chauncey Goodrich,
} Counſellors.
Simeon Baldwin, Eſq. Recording Correſponding Secretary.
Noah Webſter, jun.
John C. Smith,
Enoch Perkins,
} Correſponding Secretaries.
Joſiah Meigs, Cabinet-Keeper and Librarian.
Iſaac Beers, Treaſurer.

In the account of the laſt medical commencement in the Univerſity of Pennſylvania, the name of John S. Bellinger, one of the gentlemen admitted to the degree of Doctor of Medicine, was accidentally omitted. His Inaugural Diſſertation was on the ſubject of chronic pneumony, or pulmonary conſumption.

We are authorized to mention, that the ſecond part of Dr. Barton's "Fragments of the Natural Hiſtory of Pennſylvania," will contain a pretty full account of the quadrupeds of that State; and that he expects to publiſh it next ſpring.

Mr. Webſter's "Hiſtory of Peſtilential Diſeaſes" is printed off, and will be ready for publication in a few days.

We have juſt received from London the five firſt numbers of the "Medical and Phyſical Journal," from March to July, 1799, incluſive, conducted by T. Bradley, M. D. and A. F. M. Willich, M. D. This work comprizes a copious and valuable maſs of medical intelligence, obtained from every quarter of the civilized world. We are very ſorry that our limits do not, at preſent, allow us to make extracts from it; but ſeveral important ones will be found in our future numbers.

FOREIGN.

We are happy to announce the Intelligence of a more ſucceſsful Method of treating Pulmonary Conſumption, extracted from an intereſting Work of Dr. Beddoes, lately publiſhed, and juſt come to hand.

"HAPPILY, the ſucceſſive endeavours of Engliſh phyſicians promiſe a brilliant æra for humanity. An effectual remedy for conſumption appears to have been nearly aſcertained; and on reverting to the hiſtory of the moſt brilliant diſcovery in phyſiology, and this, the moſt uſeful diſcovery in medicine, it will appear ſingular that they ſhould have been approached by the ſame gradual ſteps. The predeceſſors of Harvey were acquainted with ſuch proofs of the circulation of the blood, that it is aſtoniſhing they did not combine them ſo as to leave no doubt in their own minds, or in the minds of others. In like manner there exiſted, before the two phyſicians who have taught its ſafe, eaſy, and effectual employment, ſuch proofs of the antiphthiſical powers of the FOX-GLOVE, that one muſt wonder its uſe had not, a number of years ago, become general.

"It is probable that certain beneficial effects of this plant, having been accidentally diſcovered, were made known, for a long time, only by oral communication. Gerard and Parkinſon, old botanical writers, mention it as an expectorant; and Dr. Withering has printed, from *Parkinſon's Herbal*, the manuſcript note of a country ſurgeon, affirming its efficacy in conſumption. In the *Family Dictionary* of Salmon, it is ſaid, upon the faith of long experience, perfectly to cure "a phthiſis or ulcer of the lungs, when all other medicines have failed, and the ſick are eſteemed paſt cure."

"Notwithſtanding the temptation which ſuch an encomium held out in ſo calamitous a diſorder, the difficulty experienced in managing the medicine, and its violent effects, occaſioned it to be abandoned, at leaſt, by the regular practitioner, till, from its efficacy in ſtimulating the languid abſorbents of the dropſical, Dr. Darwin inferred its poſſible uſe in pulmonary ulcers; and corroborated his inference by that medical miracle —a cure of confirmed conſumption—evidently wrought by this plant (Medical Tranſactions, 1785, iii. 276).

"The facts related by Dr. Darwin, and others, publiſhed by

Dr. Withering about the ſame period, ſo far overcame the apprehenſions of a large portion of the faculty, as to induce them to preſcribe fox-glove in dropſy. As the period neceſſary for its exhibition in dropſy is but ſhort, its violent effects appeared leſs intolerable. But there could be no hope of healing ulcers of the lungs in a ſhort time; and the uſe of ſo formidable a remedy in conſumption ſeemed either to be rejected by the common feelings of patient and phyſician, or elſe it was adminiſtered with a degree of timidity which could not fail to deprive it of its efficacy. In ſpitting of blood, however, and incipient conſumption, it was occaſionally ventured upon, and, as Dr. Ferriar, and, I believe, others report, with ſucceſs.

"In this ſituation the uſe of fox-glove in conſumption remained; and the ſick were left without relief, and without hope, till Dr. Drake, and Dr. Richard Fowler, led by an enlightened view of cauſe and effect, ſeem to have diſcovered what had long been the univerſal wiſh, but hardly, perhaps, the expectation of any. Dr. Drake propoſed to himſelf two objects. He hoped that the fox-glove, by promoting abſorption, would prevent that hurtful change in the ulcerous diſcharge, which he, in common with Dr. Darwin, ſuppoſes to be produced by the contact of air. At the ſame time, by powerfully retarding the action of the arterial ſyſtem, the ſecretion of matter might be diminiſhed or ſuſpended. He doubted, indeed, whether he ſhould be able, by the cautious and continued uſe of fox-glove, to render theſe conſequences ſufficiently permanent to promote a cure. He had the ſatisfaction, however, to find, in two inſtances, which he has related at large, that the pulſe could be lowered to forty ſtrokes in a minute, and the depreſſion continued till a complete and permanent cure was effected.

"Dr. Fowler's attention was directed to the fox-glove, as a remedy likely to be uſeful in phthiſis, by its almoſt uniform effect in rendering the action of the arteries more ſlow than natural, at the ſame time that it ſeems to excite the abſorbents. Diſeaſed parts of the body may be removed by depriving them of *all* ſupply of blood, and even by diminiſhing, to a certain degree, the arterial ſupply, while the abſorbents are left to act in full force. My friend hoped that this might be effected by the operation of fox-glove on tubercles in the ſubſtance of the lungs: and, proceeding upon this idea, he has been ſucceſsful in many caſes of confirmed conſumption, in

ſome of which the patients ſeemed not to have many days to live. *(Weſt-Country Contributions, Longman).* In his letters to the author, many months ago, he expreſſed the fulleſt confidence that this treatment would generally ſucceed. Both theſe phyſicians thought and acted independently of one another. In caſes of pulmonary diſeaſe, where the exiſtence of tubercles was indicated by every ſymptom, and where they ſeemed ready to break out into open ulcers, I have fully verified their obſervations; and I daily ſee many patients, in pulmonary conſumption, advancing towards recovery with ſo firm a pace, that, I hope, conſumption will, henceforward, as regularly be cured by the fox-glove, as ague by peruvian bark. Could we obtain a ſingle auxiliary for fox-glove, ſuch as we have, in many ſubſtances, for the bark, I ſhould expect that not one caſe in five would terminate as ninety-nine in an hundred have hitherto terminated. But I believe a majority of caſes will yield to ſimple fox-glove. It is evident, that no new caſes need be ſuffered to advance beyond the firſt ſtage without the application of this medicine, and few into it.

" I mean not to conceal that the fox-glove is a dangerous, which means only that it is a powerful, medicine. I ſay nothing of the manner in which it ſhould be adminiſtered; becauſe no perſon unpracticed in phyſic ſhould attempt to adminiſter it. I hope, however, that every reader of this treatiſe will inſiſt that it ſhall cautiouſly and perſeveringly be adminiſtered to his conſumptive friends. All other methods are comparatively frivolous: Moſt methods abſolutely ſo. And I know, from experience, that the fox-glove may be given, with ſafety, to hectic, and, probably, conſumptive infants, a few months old."

[*Beddoes' Eſſay on the Cauſes, early Signs, and Prevention of Pulmonary Conſumption.*

As it may gratify ſome of our readers to ſtate more particularly the manner of uſing the *fox-glove* in this diſeaſe, we think proper to add the following:—Dr. Drake preferred the *ſaturated tincture* of digitalis, of which, at the commencement of its uſe, he ordered, in one caſe, only fifteen drops, twice a day—in another twenty: in the former the doſe was gradually increaſed to one hundred drops, in the latter to ninety-ſix, with perfect ſafety. During this employment of the remedy, all the ſymptoms of irritation and fever, cough, pain, and dyſpnœa, daily grew better, and at length altogether re-

tired.—The formula prescribed by Dr. Fowler is the following: ℞ Fol. Digitalis Purpureæ recent. unc. ii. coque ex. aq. puræ ℔j ad colaturæ unc. vij ſs. & adde Tinct. Cardamom. unc. ſs. Of this decoction, Dr. Fowler generally directed half an ounce to be taken twice, thrice, and, in a few inſtances, four times in twenty-four hours.

[*Contributions to Medical and Phyſical Knowledge, collected by* Thomas Beddoes, *M. D.*

Midwifery.

In our ſecond volume, p. 111, we announced a diſcovery, ſaid to have been made by Mr. Herholdt, an eminent accoucheur at Copenhagen, that the apparent death of new-born children aroſe from the trachea being filled with a liquid, and that they might often be reſtored by merely giving them a poſition favourable to its efflux. We then expreſſed a doubt as to the correctneſs of the obſervations; becauſe it appeared impoſſible, from Dr. Goodwyn's experiments, that water could, in any caſe, enter into the trachea; and becauſe, in thoſe experiments in which it had been forced into the trachea, it had always been abſorbed, if the animal was ſuffered to live. Candour, however, now calls upon us to ſtate, that we find the following article in the Journal de Phyſique for Floreal, an. VII. [*Philoſ. Mag.*

"An important diſcovery is announced in the Medico-chirurgical Journal, publiſhed by Profeſſor Tode, of Copenhagen. Herholdt has found, on opening the bodies of ſtill-born animals, that the cavity of the tympanum was filled with the liquor of the amnios, and with phlegm (viſcous water). This fluid, after birth, iſſues by the auditory conduit, and is replaced by atmoſpheric air. This diſcovery induced him to ſuppoſe that the liquor of the amnios is introduced alſo into the canal of reſpiration before the child is born. *Experiments made at the veterinary ſchool have confirmed this hypotheſis.* Nature, in general, diſcharges this liquor; but ſometimes it is neceſſary, for that purpoſe, to employ the aſſiſtance of art. A child cannot breathe eaſily until it is freed from it. Herholdt is of opinion, that this accident occaſions more apparent deaths than is commonly believed. It is not ſufficient then to rinſe the child's throat: it muſt be placed in ſuch an attitude as will facilitate the running off of the water. The author has had the good fortune, this year, to reſtore to life twelve chil-

dren out of thirteen who were in the like ſituation. Profeſſors Abildgaard and Wiborg have confirmed this experiment by opening five puppies cut from the belly of the mother."

GREN'S EUDIOMETER.

Profeſſor Gren, in a work of his, lately publiſhed, propoſes the following method of conſtructing an eudiometer by means of phoſphorus; which, though ſlow in its operation, will give a very accurate reſult. Take a ſmall cylindric glaſs tube, ſealed at the one end, and divided from that end, by means of a ſcale, into ſufficiently ſmall equal parts; fill it with diſtilled water, plunge it into a baſon of the ſame liquid, make to aſcend a quantity of the air to be examined, and note the ſtate of the atmoſphere at the time, as indicated by the barometer and thermometer. Through a cork, ſmaller in diameter than the tube, ſtick a few needles, and place on the upper end of them a bit of pure clear phoſphorus: faſten a thread to the bottom of the cork. Introduce this cork into the lower end of the tube, with the phoſphorus uppermoſt, in which it will riſe to the ſurface of the water; ſo that the phoſphorus on the needles will come in contact with the gas in the upper end of the tube. The tube muſt now be left inverted in the veſſel filled with water, in which it muſt remain. The phoſphorus will undergo a ſlow and gradual combuſtion: by means of the thread, the cork, from time to time, may be drawn under the water to waſh off the acid produced by the combuſtion, and, adhering to the phoſphorus, to haſten the proceſs. When all the oxygen gas is conſumed, and the remaining phoſphorus is no longer obſerved to ſhine in the dark, the cork muſt be drawn out; and the obſerver muſt note, at a correſponding height of the barometer and degree of heat with that of the atmoſphere when the proceſs commenced, the quantity of azot remaining, and how much oxygen has been conſumed. [*Philoſ. Mag.*

ERRATA.

In page 140 of this number, line 6 from the bottom, for "wholly" read *very*. Line 5 from the bottom, for "aſſertions" read *aſſertion*.

P. 184, line 17 from the bottom, for "cheurinks" read *chewinks*.